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THE DEVELOPMENT OF BRITISH SCENERY.

"*It is a strange thing that the geography of the mother country has never yet been systematically worked out.*"—J. S. KELTIE, Presidential Address to Section E at the British Association at Toronto, 1897.

IT often happens that the main principles of a subject are first discovered in a region where complications exist which surround the study of that subject with many difficulties, and consideration of the growth of our knowledge of earth-sculpture illustrates this statement. The influence of the running waters of the land, first appreciated by Hutton, as the result of researches in Britain, was very fully proved by a host of our own countrymen, by reference to the facts which they acquired in this island. The complexity of the geological structure of the island prevented the laws of erosion being fully grasped by British geologists, and we owe the first additions to our knowledge of stream-sculpture to the school of American geologists and geographers, working in areas of considerable simplicity, who have supplied us with a very full account of the action of running water; prominent amongst the members of this school stands G. K. Gilbert, whose essay on denudation marks an important step in the study of erosion (1).

The complexity in the distribution of the waterways of a complex geological area may be produced (i.) by the coalescence of a number of drainage systems initiated at different times, or (ii.) by the influence of differential uplifts, and of the characters and distribution of the rocks upon

stream courses which were initiated in a geological sense simultaneously, and it is of importance to determine to which of these causes the complexity of the drainage system of Britain is due. It is generally conceded that the rivers which flow over considerable parts of Eastern England were initiated in Miocene times, or at any rate during the system of earth-movements which culminated in parts of Europe in the Miocene period, consequently, if our whole river drainage was established simultaneously, it must be referred to this date; whereas, if drainage systems were developed in our island at different times some of these must be anterior to Miocene times. It appears to be the opinion of most geologists that the latter supposition is the true one, but it does not seem to be founded upon anything beyond general impressions; a traveller amongst the old rocks of the Scotch Highlands, Cambria or Cumbria, is naturally impressed with the antiquity of the rocks on which he treads, and is unconsciously led to regard the area formed of those rocks as one which has existed as dry land through long geological times. It is desirable, therefore, that we should at the outset consider how far the geological antiquity of portions of our island *as dry land* is proved by the evidence at our disposal.

Every uplift, whether of sea-floor to form land or of pre-existing land to a higher elevation, will tend to influence the drainage and to produce watersheds or divides at first coinciding with the point or line of maximum uplift, though it depends upon the previous inclination of the uplifted rocks whether, after uplift, any definite relationship can be traced between anticlinal axes and watersheds. Now, geologists are fairly agreed as to the periods at which the rocks of the British Isles were affected by important uplifts; and those which require consideration in connection with the subject under discussion occurred at the close of Lower Palæozoic times (mainly in Devonian times), at the close of Upper Palæozoic times (especially during the Permo-Triassic period), in the middle of Cretaceous times, and in Tertiary times (especially the Miocene period) we may speak of them as the Devonian, Permian, Mid-Cretaceous

and Miocene uplifts, without attempting to indicate their maximum periods with greater exactness.

It is obvious that no drainage system can be older than the rocks which cover the area, and in Great Britain there are four large areas and several smaller ones which are unoccupied by Mesozoic rocks; the drainage of these areas may, therefore, have been initiated in Devonian or Permian times. The areas are: Scotland, the Pennine Chain, Wales, and Devon and Cornwall, and it is of interest to consider whether the river systems of these areas are of very great antiquity.

It may be remarked at the outset that if a tract has existed above the sea for long ages and not undergone any further uplift after its emergence from the sea to form high land, the rivers must have reached their base-levels of erosion and produced a plain of subaërial denudation (or *peneplain*, to use Prof. W. M. Davis' term) unless, indeed, the area has existed as a rainless region through these long periods. We cannot, therefore, suppose that the tracts above mentioned have existed as land since Palæozoic times without subsequent uplifts (which would profoundly affect, if they did not completely alter, the drainage-systems) or their present eminences would have been long ago worn away.

In Scotland the main watersheds bear no direct relation to the axes of uplift of the very ancient rocks which occupy so large a portion of its surface and the great thickness of old red sandstone and carboniferous rocks in the depression between the southern uplands and the Highlands indicates the former extension of those rocks far beyond their present limits, whilst evidence of the like extension of still later rocks has been adduced by Prof. Judd, who makes the following most suggestive remark: "In the face of these facts, I believe that it is impossible to avoid the conclusion that the whole of the north and north-western portions of the British archipelago—now sculptured by denudation into a rugged mountain-land—were, like the south and south-eastern parts of the same islands, to a great extent, if not completely, covered by sedimentary deposits,

ranging in age from the Carboniferous to the Cretaceous inclusive; and that, as a consequence, we must refer the production of the striking and very characteristic features of those Highland districts to the last great epoch of the earth's history—the Tertiary—and very largely, indeed, to the latest portion of that epoch, namely the Pliocene"(2).

Of the three other areas, Wales, which is essentially composed of Lower Palæozoic rocks, possesses a drainage radiating from the Plinlimmon district situated in a synclinal fold of these rocks and accordingly can hardly owe its present drainage directly to the Devonian uplifts, though as Carboniferous rocks nearly surround it on three sides, it might at first sight appear likely that the drainage system was a Permian one; in Devonshire the drainage is from a watershed coinciding in the main with a synclinal axis produced during the Permian uplift, and this would indicate initiation of the present drainage after Permian times; the Pennine uplift is also largely Permian, but here the watershed coincides in the main with the dominant anticlinal axis, and we might very well suppose, in the absence of other evidence, that the Pennine rivers were initiated during the Permian period of uplift.

There is, however, other evidence, which suggests a late date for the drainage of the Scotch, Welsh, Pennine and Devon areas alike, and we may briefly glance at this.

Examination of a geological map of Europe will show that the Mesozoic and Tertiary rocks of England occupy the western end of a complex syncline, with an axis directed in a general east and west direction, and the age of the rocks included in this folded system indicates that their uplift took place during the Micocene period of elevation. But the mean trend of the Mesozoic rocks of Britain is nearly north-east and south-west, for the strike approximates to a north and south direction in Linconshire and South Yorkshire (the dip being east), whilst the strike is practically east and west in the south of England. This modification in the direction of strike is undoubtedly due to the uplifts of the Mendip and Pennine systems, which have the same general direction as the strike of the newer

rocks in their vicinity, and they acted as "horsts" or barriers of harder rocks which affected the strike of the adjacent deposits. It by no means follows, however, that they were uncovered at the time that the position of the newer rocks was affected by their existence, for we actually find the continuation of the Mendip ridge still buried beneath the newer rocks underneath, and the same is almost certainly the case with the rocks of the Pennine Chain to the south, portions of which now stand out as inliers of older rock through the new Red Sandstone deposits of the central plain of England.

The Mesozoic rocks lying east of the Pennine Chain dip in such a way, that the position of the floor on which they were deposited, would, if the dip were continued westward, lie far above the summit of the Pennine Chain, and yet we find new Red Sandstone running from the west of Cumberland, down west Lancashire to Cheshire, and so into the central plain, and outliers of Lias (Rhætic) near Carlisle and near Wem in Staffordshire, far beneath the level at which they should occur, if the easterly dip were continued farther west. The conclusion seems inevitable that the Pennine Chain was uplifted in Post-Rhætic times, and therefore probably during the Mid-Cretaceous or Miocene periods of uplift, and in favour of the latter period is the existence of Cretaceous rocks in the north of Ireland, and possibly in the Irish Sea.

I have elsewhere argued in favour of the elevation of the Lake district in Miocene times (3), on account of the structure of the district, and suggested its dependence upon the formation of a laccolitic dome.[1]

The occurrence of Cretaceous gravels near Buckland Brewer, in the extreme west of Devonshire, suggests that though the Cretaceous rocks, as is well known, thin out and assume shallow water characters when traced towards

[1] In the paper alluded to, geological details were largely omitted, but the coincidence of the watershed near Shap with an anticlinal axis should have been mentioned (though well-known to British geologists), as the supposition that the Howgill Fells were uplifted after the initiation of the waterways is largely dependent upon this fact.

Devon from the east, they once covered the district. The deposition of Eocene beds at Bovey Tracey on Pre-Cretaceous rocks however shows that the district was affected by the Mid-Cretaceous uplift (or at any rate by one in Post-Cretaceous and Pre-Eocene times) if the Cretaceous rocks once extended over Devon, but the nature of the Bovey beds indicates the probability of the former extension of similar beds over Devon and Cornwall, and suggests the final great uplift of the Devon-Cornwall mass in Miocene times.

Coming now to the Welsh area, we find Triassic rocks dipping away from it on the north, east and south, and Rhætic rocks on the south, in a manner which suggests the elevation of Wales in Mesozoic or Tertiary times. We have no direct evidence of the extension of Cretaceous rocks over the area, but even if an uplift occurred in Mid-Cretaceous times which prevented the accumulation of the Cretaceous rocks over the area, the Welsh rocks were probably worn down to a peneplain before the great Miocene earth-movements.

Examination of the geology of England in fact indicates that had the Miocene tilt been in an opposite direction, giving the newer strata a westerly dip instead of an easterly one, the Highlands of Britain would be on the east side, the Mesozoic rocks would be denuded there, and the London ridge and similar ridges now buried beneath newer deposits would form a hilly country occupied by the more ancient formations, whilst the west of England and possibly Scotland and Ireland would consist of low ground formed of a peneplain of old rocks, or more probably of Triassic beds and even later deposits, possibly as modern as the Cretaceous and Eocene beds. It must be remembered that even if the west existed as land in Eocene times, the characters of the lignites and basalts of the Western Isles of Scotland and Ireland suggests their former extension as plateaux over much wider areas (4), and they may well have extended over much of the country now occupied by older rocks.

To return for a moment to Wales: The newer rocks which surround the older Palæozoic rocks of the Principality are continued across the Irish Sea and St. George's Channel,

and pass round the older Palæozoic rocks of Wicklow and Wexford, so that, if the sea were dried up, we should probably meet with a dome of old rocks, entirely surrounded by a ring of Carboniferous rocks and in parts bounded by newer formations.

I need hardly say that the proofs of the impress of the present surface features of the whole of Britain during the Miocene uplift can only be obtained as the result of much more work; I have merely endeavoured to show that Prof. Judd's view must be regarded as, at any rate, quite as probable as the one which supposes that many of our surface features date from very early times.

The movements, whatever their age, produced a general elevation in the west of England as compared with the east, and gave the main English rivers their trend to the east, whilst the subsidiary uplift of the Pennine Chain, and the formation of a syncline to the east of it, determined the Pennines as a subsidiary watershed, lying some distance to the east of the main watershed. The elevation of the lake district dome produced a subsidiary radial drainage in that region, and the Tertiary uplifts in Southern England gave rise to the Wealden drainage and to minor drainage systems situated to the west of it.

We have evidence in the existence of many submerged valleys around our coasts, and also in the interior (the latter filled with drift), that in Pre-Glacial times our land was as a whole situated at a higher level than it is at present, when indeed it formed part of the continent. The drainage to the east of England then flowed into the continuation of the Rhine; that to the south, to a river situated in the position now occupied by part of the English Channel. Soundings to the west suggest the existence of a deep river valley running from Scotland towards Cornwall, into which the rivers of the west of England, of Wales and of the east of Ireland, flowed as tributaries, and the existence of a deep channel, shallowest about the centre of the uplift of Wales, Wexford and Waterford, points to the existence of this river before the Welsh uplift, and the consequent disturbance of what would otherwise be a radial drainage,

causing the Welsh watershed to lie much east of its proper position.

The drainage established during the uplift (or uplifts) consisted primarily of rivers whose sources lay along the axes of uplift, with their subsequent and obsequent tributaries, such as the Tyne, Tees, Humber and other East Anglian rivers, and the great stream of which the Thames is a beheaded portion (5), partly of antecedent streams which continued to flow for some time across the axes of uplift (as for instance the Scotch-Cornish river on the site of St. George's Channel and the Irish Sea?), and partly of rivers flowing along synclinal depressions, as the lower parts of the Dee, the river on the site of the Severn Estuary and that part of the Thames which lies in the Eocene basin.

It is the task of the English geographer to trace the modifications which have complicated this initial river system, as well as to clear up the story of its initiation; and much work is being done in the direction of elucidating the subsequent complications. I have already referred to the work of Prof. Davis in this connection, and his paper is extremely valuable to the physical geographer, but much similar work has been done in isolated areas, especially by Ramsay, Jukes, Topley, Jukes-Browne, Strahan, Green and others (6); and we learn from Dr. Keltie's recent address to the Geographical Section of the British Association that it is proposed to carry out work of this kind in a systematic manner. He writes: "Taking the sheets of the Ordnance Survey map as a basis, it is proposed that each district should be thoroughly investigated and a complete memoir of moderate dimensions systematically compiled to accompany the sheet, in the same way that each sheet of the Geological Survey map has its printed text. It is a stupendous undertaking, that would involve many years' work, and the results of which when complete would fill many volumes. But it is worth doing; it would furnish the material for an exact and trustworthy account of the geography of Britain on any scale, and would be invaluable to the historian as well as to others dealing with subjects having any relation to the past and present geography of the land. . . . Dr. H. R. Mill

has begun operations on a limited area in Sussex. When he has completed this initial memoir, it will be for the [Geographical] Society to decide whether it can continue the enterprise, or whether it will succeed in persuading the Government to take the matter up." If either of these desirable plans be adopted, I think Dr. Keltie may feel assured that geographers will receive the heartiest support from geologists in carrying on so laudable a work.

Hitherto I have touched only on the trend of the rivers, as instrumental in determining the character of the scenery of inland districts. Many details of scenery also require study. I have already written an article in this Magazine upon Lakes (7); since that appeared the magnificent memoir on the French Lakes from the hands of M. Delabecque has been published (8), and I hope British geographers will not be contented until similar volumes on the Limnology of England have been published.

The scenery of the coast-lines is a subject which also deserves the attention of British geographers, and we may now turn to a consideration of the dominant features of the shores of our island.

The general principles of coast-formation have been stated by Gilbert in his study of the Topographic Features of Lake Shores (9). He observes that "re-entrant angles of the coast are always, and re-entrant curves are usually, places of deposition. . . . Salient angles are usually eroded, and salient curves nearly always. . . some salient angles on the contrary grow by deposition. . . ."

"It thus appears that there is a general tendency to the erosion of salients and the filling of embayments, or to the simplification of coast outlines. This tendency is illustrated not only by the shores of all lakes, but by the coasts of all oceans. In the latter case it is slightly diminished by the action of tides, which occasion currents tending to keep open the mouths of estuaries, but it is, nevertheless, the prevailing tendency."

The outcome of coast erosion and deposition is the production of concave curves usually meeting at a salient angle, and the English coast illustrates the formation of

these on different scales. Of the larger ones, we notice one extending from St. Bees Head to the north-west corner of Anglesea, a second from Anglesea to the south-west of Carnarvonshire, another from that point to Pembrokeshire, south of that a large one passing Lundy Island to the Land's End. Between the Land's End and the north foreland are a number of curves terminated by the following salient points: The Lizard, the Start, Portland, St. Anne's Head, the south of the Isle of Wight, Selsea Bill, Beechy Head, a point north-east of Hastings, Dungeness, and the South Foreland. To the north of the Foreland, we find a curve broken into by the Thames estuary and extending to the coast of Norfolk, and north of it is a feature unlike any other in England on so large a scale, namely, the great salient curve of Norfolk, which, so far as I am aware, has not been explained. Continuing northward, we again meet with a series of embayments, bounded by salient points, at Spurn Point, Flamborough Head, Whitby and Dunstanborough. It would seem that many of these larger curves are portions of an old coast line, existing during the period of elevation marked by the buried valleys. For instance, Lundy Island, situated on the curve from Pembrokeshire to Land's End, seems to represent a portion of a destroyed coast. Accordingly, we find the curves are not only modified by estuarial expanses, such as Morecambe Bay, and that lying south-east of Carnarvonshire, but a number of fjord-like indentations, indicating depression, run far inland, like the Barmouth estuary, and many of those on the coast of Devon and Cornwall.

An examination of these coast lines in detail will show important connections betwixt the geographical structure and the physical features. Morecambe Bay seems to be determined by the existence of soft new Red Sandstone rocks faulted against the older rocks and the great sweep from Cumberland to Wales is no doubt due to the existence of these rocks.

In later times, the primary curves have been modified by smaller curves. For instance, the great curve from the Start to Portland is modified by the occurrence of minor

loops as Start Bay, Torbay and others to the east, and the same thing is noticed with the other bays.

Enough has been said to show that the topography of coast lines is a fertile subject for research. Two papers have recently been published by Dr. Gulliver (10) and Mr. Vaughan Cornish (11), which bear directly upon the topography of our coasts and further work will no doubt follow.

The shape of England is roughly an isosceles triangle with a base extending from Northumberland to Cornwall and the apex on the coast of Kent. The position of the base is due to the uplift of Palæozoic rocks, to the west and north of England, whilst the position of the two sides is owing to the strike of the Mesozoic and later rocks, with a general northerly trend to the north and a westerly trend to the south, caused by the presence of the Pennine and Devon ridges or horsts. Along the strike of these Mesozoic rocks strike-rivers carved out valleys in the old continental plateau, which after depression were occupied by the North Sea and English Channel, giving England its present outline. As denudation progresses, should no further uplift occur, the Mesozoic rocks of the east and south, and the new Red Sandstone of the Central Plain and the lower parts of the Dee and Mersey basins will be denuded and our island will be broken up into an archipelago of Palæozoic rocks, bearing few or no signs of the possible modern origin of the whole as land.

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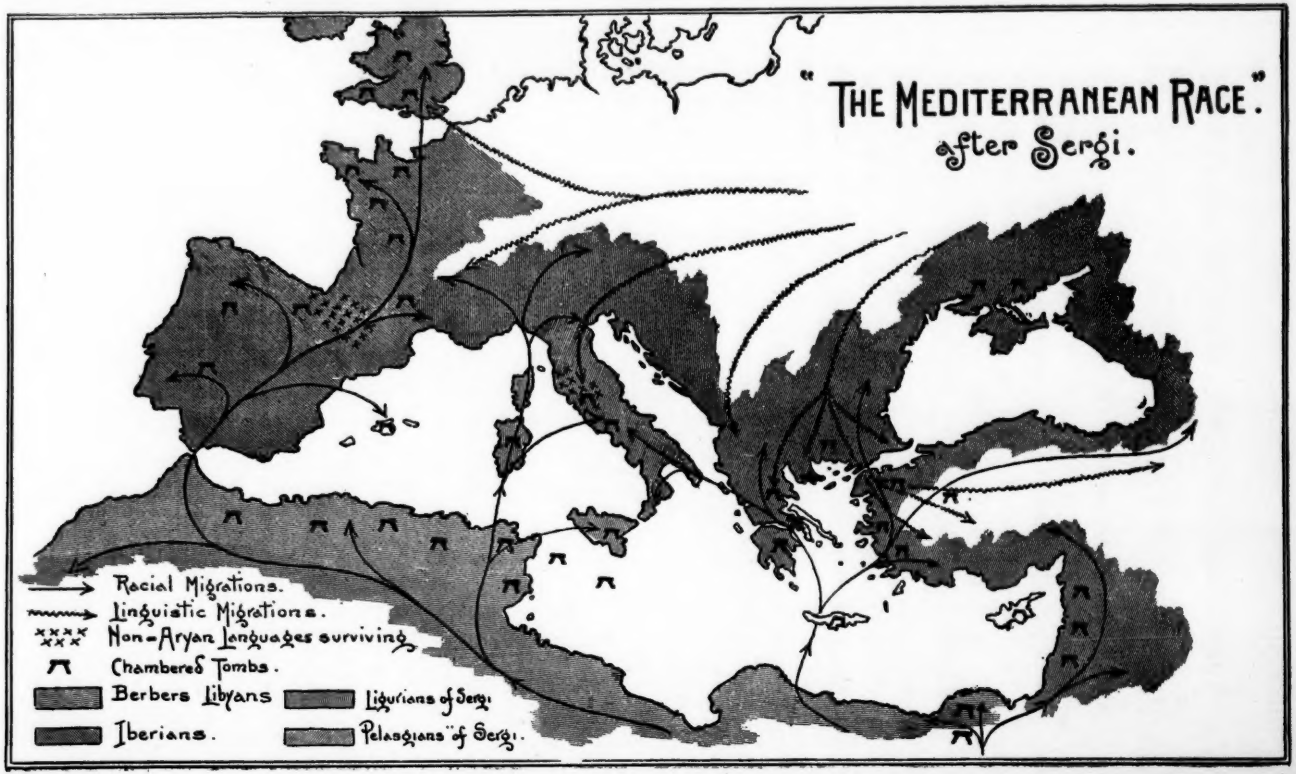
"THE MEDITERRANEAN RACE."
after Sergi.
Racial Migrations.
Linguistic Migrations.
Non-Aryan Languages surviving
Chambered Tombs.
Berbers Libyans
Iberians.
Ligurians of Sergi
Pelasgians of Sergi.

PREHISTORIC MAN IN THE EASTERN MEDITERRANEAN.

PART III.

MEDITERRANEAN ETHNOLOGY, ANCIENT AND MODERN.

THE last preceding section of this discussion closed with the question how far recent anthropological and archæological inquiries affect the validity of the Hellenic tradition, and of certain mainly philological conclusions which were commonly accepted until recently as fixing the ethnological position in early times of the inhabitants of the coasts of the Ægean and of the Eastern Mediterranean in general.

68. The Greeks themselves seem to have elaborated already in the sixth and fifth centuries B.C. a rationalised and on the whole consistent theory of their own origin, from what data we can only indicate in outline. In an explicit summary, Herodotus distinguishes four criteria of nationality: common descent, common language, common religious belief and ritual, and a common mode of life in things secular. Of the use of all these classes, examples are abundant already in his own pages, in those of Thukydides, and in surviving fragments of the rationalistic historians of the fourth century B.C.

69. But it must be remembered that to a Greek historian "community of descent" meant similarity of *traditions* of descent, unverified either by contemporary documents, or by more than the most superficial comparison of physical types: "community of language" was determined by equally superficial resemblances of individual words, traced without knowledge of phonetics, and in many cases without a working acquaintance with the non-Hellenic languages in question, or even with the remoter dialects of Greek; "community of religion," and "community of modes of life," seldom presupposed more than a certain similarity of non-essential names and forms, or such broad identity of funda-

mental purpose, as would prove nothing even between races which were really related to each other. With these drawbacks it is only to be expected that Hellenic anthropology should be inconclusive and inconsistent in detail; but for the same reason it will be the more noteworthy if we find that its main outlines still prove serviceable as a working hypothesis.

70. One cardinal belief in particular could hardly have passed into common acceptance if it had not been founded upon appreciable fact. The Greeks of the classical period firmly believed themselves to be a mixed race, and held further that each of the primary components of the mixture was itself composite; and variously composed in different districts. Stripped of its mythical and personal presentation, their view in outline was that a little leaven of a race of superior natural endowments, to which the name of Hellene more peculiarly belongs, had descended from South Thessaly, if not from Macedon, and had worked among a great mass of non-Hellenic barbarians, until the whole was leavened with Hellenic culture. Herodotus affirms distinctly that the inhabitants of Attica, and of some other districts, were not originally Hellenes, and had become so by acquiring Hellenic language and institutions. Thukydides adds that the superior race need never have been many in number, and that they owed their influence to the superiority of their civilisation, and not to any appreciable displacement of population.

71. The aboriginal pre-Hellenic stock passed under many names, among which "Pelasgian," the commonest and least vaguely conceived, passed in some measure into generic use. Prehistoric forts and other remains, mysterious cults and grotesque customs, archaic words of uncertain meaning, were explained as relics of "Pelasgian" barbarism; some Greek-speaking tribes in a backward stage of culture were thought to be still imperfectly "Hellenised"; and other remnants of earlier stocks lingering on either side of the Ægean highlands or on capes and islands, and still speaking a language which could not be recognised as Hellenic, were regarded as actual survivals of "Pelasgian," "Helegian," or "Minyan" peoples. A variety of evidence points

to a belief that the old race was of dark complexion; to an early identification of its representatives in Greece and the Ægean with the earliest stratum of population in Italy and Sicily; and to a hazy attempt to express a relationship vaguely felt to exist between these autochthonous peoples of Greece and Italy and the inhabitants of Libya, Egypt, and the Syrian coast.

72. In the strongest contrast with the autochthonous "Pelasgians," and the Ionian Greeks who seem to have been regarded as most closely akin to them, stand the immigrant Hellenes, whom Herodotus regarded as most purely represented in the Dorians, though reasons have lately been shown for holding that the Achæans had at least as good a claim to the title. Their progress was from the North, and in Peloponnese at least the last of them to arrive were never fully naturalised among the older race. Their ideal of beauty was fair and ruddy, and this type continued to assert itself in Hellenic art, side by side with the brunette type of the aborigines, at least as late as the close of the fifth century B.C., though in Græco-Roman painting it has already become rare; and in Byzantine and Romaic art, as well as among the modern Greeks, it practically dies out altogether.

73. The stories of immigrations over-sea, from Asia Minor, Phœnicia and Egypt, are not in all probability to be regarded as analogous with that of the coming of the Northern Hellenes. They invariably refer either to individual adventurers, such as Danaos or Pelops, or, as in the case of the Lybian Kyklopes, to wonder-working craftsmen summoned for a specific purpose, and are clearly attempts to explain the introduction of what appeared to be foreign elements in the prehistoric civilisation of the Ægeans, by connecting them with the arrival of semi-mythical personages.

74. Until the first quarter of the present century this traditional account passed practically unchallenged; and in most quarters survives in all its principal features, though hardly a single detailed statement has escaped critical modification.

75. During the second and third quarters of the century the principal criticisms which have passed upon it have

been based on philological evidence. The similarities between the so-called Aryan languages, from India to Wales, have been held to indicate as their common place of origin an area which is certainly not farther east than what is now Turkestan, nor farther west than what are now Scandinavia and Bohemia, and which within these sufficiently wide limits has been more exactly placed, with much probability, in the wide and fairly uniform plains of South Russia, between the Carpathian Mountains and the Caspian Sea. From this place of origin Aryan speech is inferred to have spread south-eastward to India, north-westward as far as Scandinavia and the British Islands, and south-westward into Central and Southern Europe.

76. From similarity of language has then been inferred community of blood, and a vigorous and rapidly developing Aryan race has been depicted, propagating its linguistic creed, and pressing before it aborigines, who for want of a better name have been simply described as pre-Aryans, and credited, until recently, with little or no civilisation of their own. In Greece, in particular, the migratory Hellene has been held to represent that branch of the Aryan race which broke through from the Danube Valley into the Balkan Peninsula; and Hellenic civilisation has been described in general terms as approximately Aryan in type.

77. The argument from similarity of language to community of race is, however, obviously a weak one, even when supported with secondary philological arguments from a certain community of religious ideas, mode of life, and type of civilisation among the various Aryan-spreading clans before their presumed separation. It seems an obvious remark that a language can be learnt, while physical structure cannot, and that as the widely different races which now speak Aryan languages cannot be regarded as varieties which have developed since the "migration," some of them at all events must represent non-Aryan and pre-Aryan races, who have acquired Aryan speech from the newcomers.

78. The obvious reply to this objection—that however the others may have acquired their languages, nevertheless some *one* of these distinct races must represent the original

Aryan-speaking stock—only challenges the further question: *Which*, then, is the Aryan stock? which are the non-Aryan recipients of Aryan speech? It would be disproportionate here, and it is fortunately unnecessary, to discuss the numerous attempts which have been made to answer this question. For of all the physical types which compose the population of Europe in historical and modern times, every one, the Turk only excepted, has been claimed as representing the original Aryan stock, or at least as closely akin to it.

79. We are therefore justified in putting all primarily philological hypotheses on one side; if only on the ground that none of them meet the more probable case, that the race, with which Aryan speech originated, may have been at all times few in numbers, and may further have been long since extinct; and consequently that *all* the Aryan speaking races of historic times may have, at one time or another, learnt Aryan speech, without acquiring more than the slightest tincture of Aryan blood. The ground is thus left open for a review of the whole question from a point of view primarily anthropological, and based in the first place on physical, namely morphological criteria of natural kinship between the races to be examined.

80. This is peculiarly important in the southern part of Aryan-speaking Europe, and this for two reasons. On the one hand we are here, on the philological theory, farther removed from the "Aryan Home," and separated from it by great natural barriers. The theory of wholesale migration, therefore, may here be examined in a crucial instance; and as a matter of fact the physical evidence actually gives a sufficiently coherent answer. On the other hand, it is exactly in these southern areas, which project into the Mediterranean, that the first great "Aryan" civilisations came into being, namely those of Greece and Rome which have in many respects so largely coloured current conceptions of the probable complexion of primitive "Aryan civilisation". It is therefore here that material and circumstantial evidence can be best brought to bear upon the outline of "Aryan culture," as inferred from philological

data. These civilisations, however, when examined on their material side, are found to present many features which it is impossible consistently to recognise as "Aryan". Further, in the very areas in which we find them historically, they seem to follow an essentially continuous series of development out of a thoroughly primitive and uncivilised stage, which is quite as unlike "Aryan civilisation" as is the specific culture in which they culminate. In fact, to anticipate for a moment a summary of the result, in an extract from Dr. Sergi's recent essay on the Mediterranean Race :—

> Only a few years ago, the Greeks and the Romans were thought to be actual Aryans ; and after that they were thought to have been at all events completely Aryanised. But the great discoveries which have recently been made in the Mediterranean have overturned all these theories. To-day, in spite of the fact that they became at a late stage champions of an Aryan speaking culture, the conviction is forced upon us that the oldest civilisation of the Mediterranean is not of Aryan origin, but is the product of a race composed of many blood-related peoples, who have come from a common starting-point, though in the Mediterranean area they pass under different racial names.

81. These two lines of evidence, physical and cultural, anthropological and archæological, must, however, be kept as clearly distinct from one another as from the philological evidence ; for it is in theory at all events, as easy to *learn* a mode of burial, of worship, or of metal working, as to acquire a language. Culture that is like language, taken by itself, proves, and can prove, nothing directly about Race. Like linguistic evidence, however, cultural evidence may justify important confirmatory inferences in support of a hypothesis based primarily on the morphological data.

82. The conclusions which seem to be fairly deducible from the extant remains of the first known civilisation in the Eastern Mediterranean, as to its indigenous character ; the course of its growth ; its wide influence upon the first civilisations of the Western Mediterranean, and of Central and Western Europe ; and its essential continuity, through temporary and partial eclipse, with the "Hellenic" civilisation of historic Greece, were outlined in the first paper of this series ; while the second summarised the course of recent

speculation with regard to the probable authors of this "Ægean" system of culture. It only remains, therefore, to indicate, equally briefly, the present state of our knowledge of the ethnography of the Mediterranean from the time when it first becomes traceable.

83. The Mediterranean basin, as we know it, results from the reunion of at least three detached basins by the resubmergence of the land barriers which had joined Europe and Africa in pliocene times. An approximate date for their submergence may be inferred from that of the isolation and rapid disappearance of characteristic African fauna on the north side of the reunited basin. The human occupation of the same north or European side may have been similarly interrupted by the same climatic changes which extinguished the African fauna in the same area.

84. But exceptions to this theory of a general separation of Palæolithic from Neolithic man on European sites have been recently supplied by deposits of the Baoussé Roussé caves in the Riviera, which were at first taken to be of early Neolithic date, but have since been recognised as strictly transitional; the human remains being of Neolithic type; the associated objects still quite Palæolithic in character.

85. On the south side of the Mediterranean the evidence is at present fragmentary, and derived from areas which are not yet half explored; but both a late Palæolithic and a very early matured Neolithic civilisation are already indicated at a number of points. The geographical circumstances also indicate that there existed formerly in North Africa a very much wider area of habitable country than now, and a comparatively favourable climate for a long though not exactly measurable period.

86. The geographical and archæological hints are amply borne out by the morphological data. From Somali-land and Abyssinia, through Egypt, Libya, and Mauretania, to the Canaries, the fundamental type of the native population, ancient and modern, is from east to west practically identical, though from north to south a negroid taint is increasingly perceptible, and though, besides this, marked varieties of complexion and facial feature have been re-

peatedly recorded both in Egypt from the time of the earliest portrait records (c. 3000 B.C.) and in what is now Tunis and Tripoli from the time of Herodotus onwards.

87. It has been long recognised that the predominant and longest established element in the population of Spain, namely, the type called Iberian, is very closely related to the Mauretanian, Berber, or Libyan type of the opposite African shore: and that the similar brunette and dolichocephalic element which pervades the population of the West of France and of our own country is to be regarded as a further extension of the same immigration from the South.

88. In Sicily and South Italy, a similar overflow of Libyan peoples seems to be indicated by the predominance of a type almost indistinguishable from that of the Spanish area, and already recognised in the fifth century B.C. by Thukydides or his authorities, as closely akin to it; a fact which confirms the impression that the Arab element, which here, as in Spain, has to some extent to be taken into account, is by no means wholly responsible for the frequence of North African analogies. In Italy, however, at all events, the peculiar local modifications of this type, or group of types, which occur, are in the direction of similarity to varieties which are so characteristic of the Greek peninsula and islands, that they cannot be wholly attributed to the continuous intercourse which has gone on across the lower Adriatic since the beginning of historic time.

89. The question then arises: May we infer an overflow of North African peoples into the Ægean area, similar to that which has been already noticed from Morocco into Spain and from Tunis into Sicily, to have occurred at any early period between a similar, though, it is true, less closely related pair of land prominences, namely, the Cyrennica on the south and Peloponnese prolonged through Lytheia into Crete on the north?

90. In Greece and the Ægean, unfortunately, the discoveries of human remains of early date have hitherto been comparatively rare; but the predominant types which are indicated by the published evidence correspond here also very closely with the same group of North African "Hamitic"

types which have been described already. It is true that even as early as the "Second City" at Hissarlik, one of the skulls found by Dr. Schliemann presented close resemblances to the Thracian type of Central Europe; but it may be taken as very probable that this and the rare parallel instances only represent the first beginnings of a progressive infiltration of northern brachycephalic races from beyond the mountain barrier, which has succeeded in modifying slightly the modern Greek type by an increase for example of the cephalic index from about 75 to 77; but, as in the case of the Lombards and Cormans in Italy, has hardly affected the characteristic outward type of complexion, eyes and hair.

91. East of the Ægean, the evidence for the earlier elements of the population is even more fragmentary, and at the same time the disturbances which have resulted from the Mongolian inroads of the Middle Ages are more marked. But the observations of Von Luschan and Benndorf have demonstrated a general correspondence with the same Mediterranean type; and an increasing series of skulls from Cyprus confirm that conclusion.

92. At this corner of the Levant however the whole question is complicated by the proximity and very early intrusion of races from the Syrian coast land, of the well-marked type whose distribution seems closely to correspond with the primary area of Semitic speech. This type however, as Prof. Flinders Petrie's recent observations show, is itself so closely allied to the Hamitic types of North Africa as to be difficult to distinguish from it in contiguous areas, and presents a number of intermediate forms which are probably actually half-caste.

93. Thus a survey of the whole of the Mediterranean coast-land leads to the conclusion that its earliest recognisable inhabitants and their descendants, who form the great mass of the present population, belong to a single closely connected group of races; that their earlier home is to be looked for in the formerly fertile interior of North Africa, and not improbably, as Dr. Sergi has indicated, in or near the upper valley of the Nile; and that the peninsulæ of

South Europe and Asia Minor have been peopled thence along several district routes which mainly follow the course of the pliocene land-bridges.

94. The "Mediterranean Race," thus described, has the following characteristics common to all its branches: The outer complexion is typically brown; brown skin; brown eyes, brown hair, abundant, and always more or less wavy. It is thus equally distinct from the blonde white races which bound it on the north, and from the negro races of Africa. Modifications of the brown tint are found in all branches of the race; but are conceived to be due to intermixture either with yet earlier aborigines or with subsequent intruders. The body is well proportioned, the face oval, the nose rather narrow, the orbits wide and set horizontally, the forehead high and nearly vertical, the cheekbones neither wide nor very high; the face not flattened, but if anything a little prominent in front; the neck long and well rounded, and the features mobile and expressive. It is in fact the familiar brunette type which every one recognises who has travelled in any part of the Mediterranean.

95. The forms of the skull are more variable, and have been somewhat differently interpreted by a number of investigators. To Dr. E. Sergi, however, is due a suggestion which at the same time explains the prevalence of a number of concurrent types of structure over so wide an area, and relieves us from the necessity of attributing so great importance to their divergences as has sometimes been the case. He rejects, except as a convenient *memoria technica*, the traditional and orthodox method of cranial measurement (to which he refers rather scornfully as the "anthropology of the indices") on the ground that the length and breadth measurements usually taken express merely resultants of groups of growth tendencies on the part of the various bones which compose the skull, and that such resultants may—as is obvious—be composed in a variety of ways. And certainly to classify mankind merely by the ratio of the length to the breadth of their heads, or by any other such arbitrary ratio, is little better than it would be to classify animals in general by the ratio of the length to the breadth of their whole body;

a method which would occasionally produce a surprising redistribution of affinities. Indices, it is true, are like finger-marks, an admirably compact summary of individual characteristics; but without full morphological commentary they may give very inadequate definitions of a species or variety.

96. For these empirical measurements Dr. Sergi substitutes a classification into morphological types, according to the general form of the skull. The method, in fact, is practically that of compound photography, and the principal types bear merely descriptive names such as illiptical, pentagonal, rhomboidal, or egg-shaped; qualified by specific names, either descriptive, racial, or geographical. Determined by these tests, the Mediterranean Race appears, wherever it is found, as a collocation, more or less uniformly complete, of a number of such related types: and from this it is inferred that the Race was already composite in the farthest area of origin to which it can be traced.

97. This centre is placed in Dr. Sergi's map, and, as already indicated, in the Upper Valley of the Nile, on the ground that here, among the Abyssinians, Gallas and Somalis the characteristic collocation of types is most completely exhibited; the dusky complexion of a large proportion of these races at the present day being discounted, partly by their long-continued exposure to a more tropical climate than any other branch of the race; partly by the certainty of continuous infusion of a Negroid strain from the south. It is also on this hypothesis possible to explain the very close likeness between the Eastern Hamitic and the Semetic types, and the ambiguous position and composite character of the Egyptian nationality between them; for a migration seawards down the Nile must necessarily divide at the Delta into two streams; and of these one must then move westwards along the Libyan coast, and the line of the oases, formerly much larger, which lie behind it, while the other must move eastwards into North Arabia and South Palestine; where Prof. Flinders Petrie has shown that the primitive Amorite population exhibited a physical type, and a fairly advanced civilisation almost indistinguishable from that of the Libyan element in pre-Dynastic Egypt.

98. Archæological evidence confirming this original connection of the peoples of the Mediterranean basin seems to be afforded by the extension over approximately the same area and from Neolithic times onwards of the custom of burial in sepulchral chambers, either rock-hewn or constructed on or close below a level surface ; and in the latter case covered by a mound of earth or stones. As typical examples of this widespread type of interment, and in illustration of the diverse local developments which it has undergone, we may cite alike the "mastabas" and Pyramids of Egypt, the dolmens and chambered tumuli of Tunis and Algeria, of Spain, the West of France, and our own islands, the Nuraghe of Sardinia, the "tombe a camera" of Etruria, the "beehive" tombs of Greece and the Ægean, the chambered tumuli of Karia, Lydia, and Phrygia, and perhaps also, according to Dr. Sergi, the Kurgani of Southern Russia. For these monuments not only range over approximately the same area as the race in question, and accompany the development of a primarily homogeneous civilisation, but are always found to be tenanted by representatives of the same physical type, wherever their contents are sufficiently well preserved.

99. In the same way, a number of independent investigations of the "pre-Aryan" languages which survive into historic times within the same area seem to converge upon the conclusion that the Mediterranean basin corresponds also to an early linguistic province.

100. Meanwhile, alongside of this whole group of inquiries tending to establish the essential unity, and independent native development of the Mediterranean province, a similar series of conclusions are taking shape, which bear directly upon the second component recognised in the traditional scheme, with which Ægean and eventually all Mediterranean ethnology historically begins; and with an almost identical influence upon that early hypothesis In this restricted and qualified field the Aryan hypothesis has proved a valuable working suggestion, especially since it became probable that not merely the Italic and Hellenic groups of languages were of kindred northern origin, but that the latter was intimately related with the extinct

language of Thrace as an intermediary to a group of languages certainly intrusive but long dominant in Asia Minor; of which Phrygian and Armenian are the best preserved examples.

101. Moreover on this side of the Ægean also the linguistic invasion coincides with an even better preserved tradition of a recent but already evanescent overflow of highly endowed and politically dominant clans from South-east Europe into the Anatolian coastland, and even on to the Phrygian plateau. More than this, the overflow in question was not yet at an end even at the opening of Hellenic history; in the Homeric "Catalogue of the Allies of Priam," a document which in every other particular runs in correct geographical order, Bithynia, which is practically Thrace-in-Asia is significantly omitted: and the Kimmerian invasion of Asia Minor in the seventh century B.C. can only be satisfactorily interpreted as originating, like the Bithynian invasion, and probably in the closest connection with it, from South-east Europe. The inroad of the Gauls in the third century B.C., which resulted in the superposition of Galatia upon the south-east part of immigrant Phrygia, is of course an almost exact repetition of the same series of events.

102. Archæological evidence also occurs in the same sense, though it is fragmentary and for the most part still much disputed. That the culture province of the Danubian basin, from the first moment of the trade in tin and amber across the mountain barrier, exercised an appreciable reflex influence upon the civilisation of the Ægean area, has been now for some years undisputed; and it is highly probable that, as in the later examples of Greece, of Rome, of provincial Gaul, and, as Mr. Evans has suggested, of Celtic Ireland, this commercial intercourse first revealed to the waking intelligence and restless energy of the peoples beyond the Balkans, as afterwards beyond the Alps and the Rhine, the resources and amenities of the coastlands of the Mediterranean; whereas on the Aryan paradox, *E Borea lux*, no motive is supplied for these southward immigrations into lands which *ex hypothesi* must have been still barbarous. On this side the recent papers of M. Salomon Reinach,

Mr. Evans and Prof. Ridgeway summarise fully the present acutely transitional character of current views on the ethnology and early civilisation of the Ægean, the two keynotes of which, as has been indicated more than once in the preceding paragraphs, are the insistence on the original and independent character of the Mediterranean province, and particularly of the Ægean area of it; and with regard to the admitted subsequent intrusion of ethnic, linguistic and cultural elements from the north, a return to a general presentment of the evidence, which almost literally coincides with that of the Hellenic anthropology of the sixth and fifth centuries B.C.

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THE EXTRACTION OF GOLD AND THE CYANIDE PROCESS.

PROGRESS in the metallurgy of gold has of late undergone a complete change. The empirical stage of the art persisted long after the importance of the scientific foundation underlying most other industries had been fully recognised. The victory of common sense over ignorance or prejudice has been a slow one, and for many years the "practical" man continued to scorn his "theoretical" contemporary, until at length the two became united in the scientific metallurgists who are to-day engaged in all parts of the world in the production and purification of the precious metals. Research work, under conditions likely to lead to useful results, and conducted by men who are familiar with the problems to be solved, has been rendered possible by the huge scale on which operations are now conducted in many parts of the world. Parties of diggers working surface deposits have no time or money to investigate knotty points in the metallurgy of gold, but it is otherwise with the great companies which deal with the South African reefs, and the rate of progress in scientific metallurgy has been greatly increased by the means which these wealthy corporations have been able to provide. Moreover when chemistry makes a present of some useful fact to her technical sister, a whole army of workers now fall upon it, dissect it, amplify it, and soon enrich pure science with many return gifts.

The way in which scientific discoveries and their technical applications may react on each other is well exemplified in the history of the work which has been done on the solvent action of potassium cyanide on gold. Dr. Wright of Birmingham discovered this action in 1840, and it was mentioned in a patent specification relating to electroplating taken out by Elkington in the same year. But though Bagration[1] in 1843, Elsner[2] in 1846, and

[1] *Bull. de l'Acad. des Sciences de St. Petersbourg* (1843), vol. ii., p. 136.
[2] *Erdm. Journ. Prak. Chem.*, vol. xxxvii. (1846), pp. 441-446.

Faraday[1] in 1857 made long series of experiments on the subject, the discovery remained for nearly half a century one of the unused and apparently useless chemical data which help to fill text-books for a time and are then omitted as of no interest and finally almost forgotten. When it was found, as the result of repeated trials in 1886, that alkaline cyanides in dilute solution are fairly stable substances, their solvent action on gold became of value to mankind, and since then an ever-increasing army of workers has been carefully experimenting on the action of cyanide not only on gold but on other metals, on sulphides, oxides, and silicates, on wood and a hundred other substances, with the result that the data accumulated would fill a volume by themselves.

Much light has been thrown, for example, on the exact mechanism of the chemical change which ensues when cyanides act on gold. Elsner found that the air at the top of an inverted test-tube containing gold dipping into a solution of cyanide of potassium had lost its oxygen after twenty-four hours, and considered that this had been consumed in dissolving the gold, although the oxidation of cyanide to cyanate might have accounted for its disappearance. Faraday discovered that if gold leaf is floated on the surface of a solution of cyanide it is dissolved many times more quickly than if it is completely immersed and so protected from the air. It was subsequently proved by Maclaurin[2] that pure gold is not soluble in a solution of pure cyanide if oxygen is completely excluded, and that dissolution is greatly increased if the liquid is thoroughly aërated and especially if oxygen is continually bubbled through it. Much evidence was afforded by him in support of the correctness of the equation :—

$$4Au + 8KCy + O_2 + 2H_2O = 4KAuCy_2 + 4KOH.$$

Nevertheless this equation does not represent the whole of the chemical action, as a substance reacting like hydroxyl seems to be produced. To explain this, G. Bodländer of Clausthal puts forward the equation :—

[1] *Roy. Inst. Proc.*, vol. ii., p. 308.

[2] *Jour. Chem. Soc.* (1893), vol. 63, p. 724.

(1) $2Au+4KCy+2H_2O+O_2=2KAuCy_2+2KOH+H_2O_2$
followed by—

(2) $H_2O_2 + 2Au + 4KCy = 2KAuCy_2 + 2KOH.$

In place of this Bettel[1] suggests the following :—

(1) $2Au+6KCy+O_2+2H_2O=KAuCy_2+KAuCy_4+4KOH$

(2) $KAuCy_4 + 2Au + 2KCy = 3KAuCy_2.$

Even if such actions occur, however, it is possible that they are limited to an insignificant part of the whole mass.

However this may be, it is fully proved that gold cannot directly displace potassium or sodium in alkaline cyanides, the liberation of hydrogen in the liquid never having been observed, whilst conversely the displacement of gold by metallic potassium is readily effected, and is complete. It is necessary that some substance should be present having a strong affinity for potassium, so as to unite with it, in order that gold may be dissolved by cyanide. The usual agent employed is oxygen, but it is not necessary that it should be in the free state, many substances containing it loosely combined being efficient substitutes. Mac Arthur, indeed, has cited experiments[2] to show that gold in ores can be dissolved by potassium cyanide in the absence of oxygen, and Bettel[3] found this to be the case, if the crushed ore contains basic ferric sulphate (a common constituent where oxidised pyrites are present), by which potassium ferricyanide is formed, thus :—

$$Fe_2(SO_4)_3 + 12KCy = 3K_2SO_4 + K_6Fe_2Cy_{12}.$$

The addition of other oxidising agents to expedite the action of cyanide has been suggested on all sides, and the results of work in this direction have been the basis of several variations in the original process. Some of the most interesting results are those obtained by Bettel and Marais in 1894.[4] They first removed all solvent power from a cyanide solution by expelling the dissolved oxygen with a current of hydrogen, and then added various oxidising agents and observed the effects produced by them. Under these circumstances, neither potassium bichromate,

[1] *South African Mining Journal*, 8th May, 1897.
[2] *Jour. Soc. Chem. Ind.* (1890), p. 7. [3] *Loc. cit. ant.*
[4] *Proc. Chem. and Met. Soc. of S. Africa*, May, 1897.

chromate, chlorate, perchlorate, nitrate nor nitrite enabled the solution to dissolve gold leaf, and ferric hydrate and bleaching powder were also without effect. The addition of pyrolusite gave a doubtful result, and lead dioxide caused very slow dissolution of the gold. Doubtless the presence of hydrogen tended to hinder these substances from assisting the cyanide, but under similar conditions if ferric chloride, chlorine, or iodine dissolved in potassium iodide were added, gold was dissolved slowly. The action was more decided if an addition was made of potassium ferricyanide or permanganate, sodium dioxide, hydrogen peroxide or barium dioxide. Finally, gold dissolved rapidly when bromine was added.

It is probable that ferric chloride and the halogens act without any intervention of oxygen, the superfluous potassium being converted directly into a haloid salt according to some such equation as the following :—

$$2\,Au + 4KCy + Cl_2 = 2KAuCy_2 + 2KCl,$$

some cyanide of chlorine being formed in the portions of the solution where gold is not present.

The essential point is that some substance, having a strong affinity for potassium, must be present to assist in the displacement of that element by gold in the cyanide molecule. Whether the removal of the potassium is effected by oxygen or a halogen is immaterial if the mass or concentration of one of these agents is sufficient, and although their minimum effective concentration is unknown it is clear that, as cyanide is not a solvent without the aid of one of them, it is useless to increase the strength of cyanide without a corresponding increase in the amount of the oxidiser. This is the reason why strong solutions of cyanide are not better solvents of gold than weak ones, unless means are taken to increase the quantity of available oxygen. Moreover, since cyanide acts directly on the sulphides of the base metals (which are usually present in gold ores), without requiring the assistance of oxygen, a greater waste of the solvent results in proportion as the solution is stronger. The reason for the necessity of circulating the solution in treating ores is also clear, as the very

small quantity of oxygen (amounting to only about 0·4 per cent. by volume in good solutions in the Transvaal) in the neighbourhood of a particle of gold would soon be exhausted, long before the cyanide could be saturated with gold.

It is not necessary to take special means to add oxygen to cyanide solutions when ores, such as Transvaal tailings, poor in gold and free from reducing agents, are in course of treatment. Enough air is entangled in the ore or dissolved in the solution for all practical purposes. When pyritic ores are treated the supply of oxygen is exhausted before the whole of the gold is dissolved, and it has been found desirable to resort to the "double treatment," as for example at the Primrose Works at Johannesburg, where pyritic tailings are leached in two vats in succession, the process of draining dry and transferring the ore being chiefly beneficial on account of the aëration that is thus effected. When concentrates rich in sulphides came to be treated, the difficulties of supplying sufficient oxygen were found to be still greater. The gold dissolves with such extreme slowness that treatment occupied two or three weeks, even if the ore was drained dry at intervals and stirred up.

It has long been remarked that a small percentage of a soluble sulphide present in the cyanide solution greatly delays the dissolution of gold. Doubtless this is partly owing to the abstraction of oxygen from the solution by the sulphide, for gold sulphide is freely soluble in KCy so that the surface of the metal is kept free from sulphide if the cyanide is not too dilute. Bettel however points out[1] that silver sulphide is far less soluble than gold sulphide, and that if native gold is alloyed with 20 per cent. of silver, no uncommon occurrence, a film almost insoluble in dilute cyanide solutions may be formed. It is certain that some specimens of gold leaf dissolve with great difficulty if they have been previously dipped in sulphide solutions, or if traces of soluble sulphides or sulpho-cyanides are present in the solution. The difficulty disappears if the sulphides are removed, either by being precipitated with lead salts, or by the action of certain oxidisers.

[1] *South African Mining Journal*, 8th May, 1897.

The limit of difficulty of dissolution is reached when the ore contains some rapid absorbent of oxygen such as ferrous sulphide, when, instead of encountering delay the operator finds that the gold is not dissolved at all. In such a case it is necessary to saturate the substance completely with an excess of oxygen before applying the cyanide solution. It is in this way that Caldecott solved the problem of getting into solution the gold contained in accumulated slimes on the Rand.[1] The gold in slimes fresh from the battery is readily soluble in cyanide solution, being in a very fine state of subdivision, but it is quite otherwise with slimes which have accumulated in dams and settling pits and have been exposed to the weather for some time. Under these conditions the iron pyrites is rapidly decomposed into ferrous sulphide and free sulphur, the impervious nature of the materials preventing free access of air which would result in the formation of sulphates and free acid. To prepare these slimes for treatment Caldecott supplies oxygen artificially in the form of air delivered from a perforated pipe fixed near the bottom of the agitation vat containing the pulp. Aeration of the slimes has now been regularly used since the end of 1896 at the Rand Central Ore Reduction Company's Works, accelerated when this operation takes too long or when much organic matter is present with from two to eight ounces of permanganate of potash per ton of dry slimes. After the presence of ferrous sulphide can no longer be detected in the slimes, the aëration is still carried on for an hour or more, in order to oxidise any ferrous hydrate remaining and cyanide is then added, the gold being now found to be readily soluble.

Such treatment represents what is necessary in an extreme case, and the addition of potassium permanganate or sodium dioxide is often made in the United States and elsewhere merely to increase the rate of action of cyanide. In the Sulman Teed process, now in operation on arsenical pyritic ore at Deloro in Canada,[2] oxygen is dispensed with

[1] *Proc. of the Chem. and Met. Soc. of S. Africa*, 17th July, 1897.

[2] See paper by Hugh K. Picard read at the London meeting of the Federated Institution of Mining Engineers, May, 1898.

and cyanide of bromine added, when potassium bromide instead of the hydrate is produced, the action being expressed by the equation

$$CyBr + 3KCy + 2Au = 2KAuCy_2 + KBr.$$

As already observed, the same direct removal of potassium is probably effected when the halogens are added to cyanide solutions. But, on the other hand, Clennel has advanced some evidence[1] of the formation of considerable quantities of hydrocyanic acid, when bromide of cyanogen is added to potassium cyanide, and a corresponding amount of oxygen would thus be rendered available either to form cyanates or potash, so that the action of bromine would after all be merely one of oxidation.

Although the alkali metals are positive to gold in cyanide solutions there are others which can be directly displaced by both gold and silver. Mercury is one of these, the solvent action of the double cyanide of potassium and mercury being independent of the presence of any substance having an affinity for potassium, and this double salt is sometimes used on refractory ores in which the gold is difficult to get into solution. Other elements, such as iron, lead and carbon, which are negative to gold in cyanide solutions, also increase the rate at which it is dissolved if in contact with it, but the difficulty of obtaining contact is too great for these substances to be of any practical value.

In general the various quickeners of the action of cyanide are unnecessary with simple ores, as for example in the Transvaal, where the leaching of the tailings occupies two or three days owing to mechanical difficulties, so that nothing is gained by reducing by a few hours the time necessary to dissolve the gold. On the other hand an abundant supply of oxygen in almost any form increases the destruction of the cyanide in various ways, so that the cost may become almost prohibitive.

The consumption of cyanide has been undergoing reduction by repeated steps during the last few years by means of decrease in the strength of the solution. Beginning

[1] *Proc. Chem. and Met. Soc. of S. Africa*, vol. i., p. 127.

with 1 per cent. solutions the metallurgists of the Rand soon cut this down to 0·3 or 0·4 per cent., and the limit seems now to have been reached in the treatment of slimes, in which solutions containing 0·001 per cent. are in use, or only one-third of an ounce of cyanide in a ton of water.

The question of dilution is intimately bound up with that of precipitation. In the early days when the zinc process only was practised, it was found that the gold was precipitated more rapidly and completely when a considerable excess of free cyanide was present, and it was common to make up the strength of the solution coming from the leaching vats before passing it through the zinc boxes. In particular, pure sheet zinc is quite unable to precipitate gold in the absence of free potassium cyanide, although the filiform zinc, consisting of thin threads, often as much as a yard long and $\frac{1}{20}$th inch wide, slowly extracts gold from the same solution.[1] Christy suggests[2] that the cause of this is that hydrogen is set free by the first action of the aurocyanide of potassium on the zinc, thus:—

$$2KAuCy_2 + 2Zn + 2H_2O = 2KOH + H_2 + 2ZnCy_2 + 2Au$$
$$(+ \text{ circ. } 45 \text{ calories}).$$

This equation is equivalent to stating that the K ions in the aurocyanide are replaced by zinc, as follows:—

$$2(K + AuCy_2) + Zn = (Zn + 2AuCy_2) + 2K$$

followed by a rearrangement of the molecule of zinc-gold-cyanide, and the replacement of the gold by another atom of zinc. This mechanism of change seems the more probable when it is remembered that the K ions in KCy itself are displaced by zinc in the presence of water, and that the whole reaction is more strongly exothermic than that of simple displacement of gold by zinc.

Assuming, then, that hydrogen and zinc cyanide are set free as above, imperceptible layers of both would be formed on the surface of the sheet zinc, which would thus be soon protected from further action. Hydrogen would also be set free on the surface of the thread zinc, but in this case the

[1] "The Precipitation of Gold by Zinc-Thread from Dilute and Foul Cyanide Solutions," by A. James, *Am. Inst. Mag. Eng.*, Feb., 1879.

[2] "The Solution and Precipitation of Gold," *ibid.*, 1896.

ragged edges of the shavings would assist the gas to form into bubbles and become detached, so that less "polarisation" would occur, but it must be admitted that the well-known evolution of hydrogen in the zinc boxes may be explained in other ways. The layer of $ZnCy_2$ is at once dissolved if potassium cyanide is present, and in its absence potash has a similar but less active effect, potassium zincate being formed.

The view, however, formed too hastily without sufficient experiment, that if the strength of free cyanide falls below about 0·2 per cent. precipitation is impracticable, has now been abandoned except by the advocates of rival precipitation processes. McBride obtained satisfactory precipitation by zinc at the Glencairn Mine[1] with solutions containing as little as 0·025 to 0·03 per cent. KCy, and James showed the conditions to be observed in such cases, one of them being that the solution must be in contact with the zinc for at least an hour, so that 130 cubic feet of shavings (weighing 1 lb. per cubic foot) are required to treat 100 tons of solution in twenty-four hours.

It must, of course, be borne in mind that the zinc used is not pure, and that the small percentage of lead usually contained in it has a powerful effect in increasing its action, a couple being formed with the zinc as the positive element. The gold-zinc couple, produced after precipitation has proceeded for some time, is probably also of value, but on the other hand Goyder has shown[2] that the presence of iron in contact with the zinc is injurious, checking the rate of precipitation of the gold and increasing the waste of zinc and the tendency for the formation of $ZnCy_2$ on the threads. Iron screws should therefore not be used in the precipitation boxes. Ehrmann more recently[3] tried the copper-zinc couple and found it most efficacious in hot solutions, but even in the cold his results were striking. For example, a cold solution containing 0·017 per cent. KCy and seven dwts. of gold per ton had 61 per cent. of the latter pre-

[1] *Proc. of the Chem. and Met. Soc. of S. Africa*, vol. i., p. 289, 1897.

[2] *Chem. News*, vol. lxxiii. (1896), p. 272.

[3] *Chem. and Met. Soc. of S Africa*, 17th April, 1897.

cipitated by zinc in twenty-four hours, and, under precisely similar conditions, 98·67 per cent. of the gold was precipitated by the copper-zinc couple, the residual liquor containing only two grains of gold per ton.

Apart from the zinc process, the only other method in extensive use of recovering the gold is that of Siemens and Halske, in which a current of electricity is passed from iron anodes to sheet-lead cathodes through the solution. The electrodes are placed from $1\frac{1}{2}$ to 3 inches apart and a current of four volts, and from 0·03 to 0·05 ampere is used. The gold is precipitated on the lead, which, after a run of one or two months, is removed, melted and cupelled. The chief advantage derived is that precipitation is possible from very dilute solutions containing from 0·001 to 0·1 per cent. KCy, and as these are almost, if not quite as effective as stronger solutions in dissolving gold from its ores, while suffering less loss from decomposition of the cyanide, some saving is effected by their use, estimated by Bettel to be about $\frac{1}{3}$ lb. of cyanide per ton of tailings. One of the main difficulties in electrical precipitation lies in the great resistance offered to the passage of the current by the dilute solutions employed. In order to reduce this as far as possible the electrodes must be of large size and the distance between them small, so that the use of mercury for the cathode, otherwise marked out as the most suitable substance, is not practicable.

The ions of aurocyanide of potassium are K and $AuCy_2$. As the result of electrolysis, therefore, potassium is set free at the lead cathode, where it attacks the water forming potash and hydrogen; at the same time the gold in the double cyanide is displaced and precipitated, both by the potassium and the nascent hydrogen. Such hydrocyanic acid as may be formed is neutralised by the potash. Meanwhile the anion, $AuCy_2$, is set free at the anode, but is at once split up into AuCy and Cy; the latter unites with the iron, forming cyanides which become converted into Prussian blue, and are also oxidised in part, forming ferric oxide, and the cyanide of gold is partly precipitated in this substance, although if enough free potassium cyanide is present it may

be kept in solution. When precipitated the gold cyanide can afterwards be recovered by treatment with KCy. It is clear therefore that the solutions must be circulated, and also that the more free cyanide is present the greater is the percentage of gold precipitated on the cathode.

The value of electrical precipitation of gold from cyanide solutions has in no case been more clearly demonstrated than in the treatment of slimed ore, and it may fairly be claimed that the problem of its treatment, which for the past three years has taxed the ingenuity of the chemists and engineers of the Rand, could not have been solved without the aid of Siemens and Halske's process.

When the Rand ore is crushed in stamp batteries, part of it is reduced to an excessively fine state of division, which settles very slowly in still water, and if allowed to remain in the tailings prevents leaching in large vats from being carried on effectively by forming impervious layers in the sand. It was found necessary therefore in South Africa to separate the slimes from the rest of the tailings by running the pulp from the batteries into large vats full of water, when the coarser particles of ore sink and the slimes overflow at the top. The slimes amount to about 30 per cent. of the dry weight of the ore, and though their assay value is not high, nevertheless when thousands of tons of them were run to waste every day the aggregate loss was very great. The problem of the extraction of the gold from this material was therefore one of great importance. It was attacked by many able metallurgists in the Transvaal, and the economic success of the treatment now adopted as the result of their labours will have a considerable effect on the future prosperity of the gold-field. The method of treatment is as follows[1]:—

The slimes as received from the mill are suspended in water, of which they form about 2½ per cent. Milk of lime is added to coagulate the fine ore so as to assist it to settle, and part of the water is then got rid of by Spitzkasten followed by settlement in continuous-overflow pits. The settled slimes, containing about 50 per cent. of water, are

[1] Address by Chas. Butters, *Journ. Chem. and Met. Soc. of South Africa*, Feb., 1898.

then ready for cyaniding, and the superfluous water is pumped back for use again in the batteries.

The difficulties in further treatment have little to do with getting the gold into solution, but are due to the impossibility of passing liquids through the clayey mass. It is necessary to wash by decantation, and the amount of liquid thus obtained is from six to ten tons per ton of slimes, the solutions containing only from 0·01 to 0·001 per cent. of cyanide and, after the ore has been treated by them, from six to twenty-four grains of gold per ton, and the recovery of gold from these solutions was an entirely new problem for the metallurgists.

Several methods are in operation in the Transvaal for the solution of the gold, but the best is probably that in which the slimes together with cyanide solution are passed by a centrifugal pump from one tank to another. In this passage of the pulp through the pump and pipes, air is drawn in through the joints and glands, and is introduced into the suction of the centrifugal pump. In this way a large amount of oxygen is supplied to the pulp whilst it is undergoing the most perfect agitation in its passage through the pipes, and further aëration is effected when the stream falls into the vat, carrying air down with it. H. T. Durant was responsible for the final modifications in this system, which had been introduced by J. R. Williams, and the time required for the solution of the gold is now very short, being mainly effected during the passage of the pulp from one vat to the other.

It is found to be very undesirable to attempt to reduce the amount of solution added at one time to the slimes. In thick slimes, even lumps of cyanide become coated over and protected from the solvent action of the water, although rapidly dissolved if there are four or five tons of water to one ton of slimes, and the same effect is observed in dissolving gold. To enrich the solution, however, two charges of slimes are treated in succession by it before it is passed to the precipitation plant. After settling for twenty-four hours the solutions are decanted, and at the Rand Central Ore Reduction Company's works the slimes are finally transferred to vats 50 feet in diameter and 16 feet deep, holding nearly

1000 tons, where further settling takes place, and the slimes, discharged once a week, are found to retain only about 40 per cent. of moisture. The settling capacity of these vats has lately been increased by the delivery of the stream of pulp through a pipe of large diameter carried half-way to the bottom of the vat, so that the disturbance produced by additions is less marked and the settling proceeds without interruption. The settled slimes are discharged from these vats by a jet of water under 200 lb. pressure per square inch, which rapidly disintegrates the clayey mass, so that 500 tons weight of slimes can be discharged in three hours with little labour and at a low cost.

In the electrolytic precipitation, it is necessary to free the liquid from all solid suspended particles to prevent the electrodes from being coated with slime. This is done by filtering through sand or by prolonged settlement, but the clearness of the solution also depends on the solubility of the anode. By keeping the current density below 0·04 amperes per square foot, however, very little corrosion of the iron anodes takes place, a plate $\frac{3}{16}$ inch thick being estimated by Butters to last five years, while with Andreoli's peroxide of lead anode, practically no corrosion of the anode takes place at all.

Recent details of the exact cost of treatment of slimes are not available, but J. R. Williams gives it for the months of April and May, 1897, at the Crown Reef Mill as 3s. 9d. per ton including maintenance. Of this amount, cyanide cost 5½d., lime 6d., and royalty for the use of electrical precipitation nearly 5d. The Crown Reef Company, by means of its stamp mill, cyanide plant and slimes plant is now recovering over 90 per cent. of the total gold contained in the ore, at a cost of less than 6s. per ton of original ore.

The treatment of slimes, the latest development of the cyanide process, is a sign of the new order of things in the metallurgy of gold. Worked out with the greatest patience and skill by direct experiment on the substances to be treated, and without any thought of exclusive advantage to its devisers, its main features have been at once published for the free use of the rest of the world, and will probably pass into general use with little delay.

T. K. Rose.

THE NATURE OF ALTERNATION OF GENERATIONS IN ARCHEGONIATE PLANTS.

AN HISTORICAL SKETCH.

THERE are few scientific terms the meaning of which does not become more or less modified with the progress of investigation. Cases of the complete loss of the original significance are, however, less frequent, and when they occur usually involve a period of transition before the the use of the term again becomes definitely fixed. The phrase "alternation of generations" as applied to plants has undergone such a change of meaning: this necessitates a brief retrospect of its use in the past fifty years before attempting to consider recent progress in our knowledge and opinions regarding the phenomena now denoted by it.

The term was introduced by Steenstrup,[1] in 1845, to denote "the remarkable and till now inexplicable natural phenomenon of an animal producing an offspring which at no time resembles its parent, but which on the other hand itself brings forth a progeny which returns in its form and nature to the parent animal". Plants are not referred to until the last page of this monograph which deals with various groups of invertebrata, and then the succession of vegetative shoots culminating in the flower or reproductive shoot is recognised as the corresponding phenomenon. The diagrammatic representation of a similar view in the frontispiece of Owen's classic work on *Parthenogenesis* may be referred to in illustration.[2] That the importance of the succession of sexual and spore-bearing forms in the life history was not recognised can be readily understood, when the fragmentary nature of the available data is remembered. Observations pointing to the discontinuity between these two stages in Bryophyta and Pteridophyta indeed existed, but a comparative treatment was wanting. Even in 1851, Braun[3] only uses the term alternation in the sense in which

[1] Steenstrup. [2] Owen. [3] Braun (1).

it had been employed by Steenstrup. He, however, discusses at considerable length the life history of mosses and ferns, in which fertilisation is regarded as falling in the middle of the cycle of development, "becoming the means of transition from a lower to a higher stage of the metamorphosis". In a work[1] published two years later similar views with regard to the importance of this alternation of shoots are elaborated in detail; the importance of the sexual or asexual mode of reproduction is, however, recognised, though the cryptogams are intentionally left out of consideration.

But in the *Vergleichende Untersuchungen*[2] the life histories of the main groups of archegoniate plants were described in detail and treated comparatively; from this time the importance of the regular alternation of a sexual and spore-bearing generation has been beyond question. In this work, and even in the *Higher Cryptogamia*"[3] published in 1862, Hofmeister made no remarks on the nature of the alternation of generations, the essential unity of which throughout Bryophytes, Pteridophytes and Gymnosperms he clearly demonstrated; neither did he extend the comparison to Thallophytes.

In the interval between these two works of Hofmeister the materials for this comparison had accumulated as the life histories of the Algæ became accurately known. In 1856 Pringsheim had pointed out that *Œdogonium* and *Coleochæte*, in both of which the zygote divides into a number of cells, approached the level of development of the moss, and had compared the many celled fruits of the latter genus with the sporogonium of *Riccia*.[4] This view is still further developed in later papers[5] and an important distinction is drawn between the alternation of shoots and true alternation of generations, the succession of individuals by asexual reproduction in *Coleochæte* being regarded as the equivalent of the former. "We see in the *Coleochæteæ*, just as in the pleurocarpic mosses, the succession of shoots or succession of generations along with the form

[1] Braun (2). [2] Hofmeister (1). [3] *Ibid.* (2).
[4] Pringsheim (1). [5] *Ibid.* (2), (3).

of alternation of generations which expresses itself in the fruit-formation."

At this time descent was not a recognised factor in morphological comparison. In reading the clear statements of fact and the definite conclusions arrived at on comparative grounds in the works of Pringsheim and Hofmeister this circumstance is readily forgotten. It applies however to papers published for some years after the first edition of the *Origin of Species*. The importance of descent in such morphological questions was recognised and insisted upon by Haeckel in 1866.[1] He discussed alternation of generations in plants and animals and, though some of his comparisons were misleading, the main conclusions so far as they relate to plants must be mentioned on account of their bearing on later theories. In Bryophyta and Pteridophyta, which he grouped together as Prothallophyta, Haeckel recognised true alternation (Metagenesis), but he compared the protonema with the fern prothallus, and the leafy moss plant together with the sporogonium to the fern-plant. Except for isolated cases of reproduction by separable bulbils he did not consider that true alternation was exhibited by Phanerogams. In the latter the succession of vegetative and reproductive shoots was distinguished from Metagenesis under the name of Strophogenesis or succession of generations.

The influence of Haeckel's views on alternation is clearly traceable in a paper by Strasburger,[2] which appeared some years later; the conclusions are of interest from a historical point of view, although they are not maintained in later works by this author. He distinguished as true alternation of generations (Metagenesis) the succession in the life history of two or more genealogical individuals which have become more or less unlike. This is recognised as existing in the vegetable kingdom only among Thallophytes. From the Mosses onwards the alternation is considered to be of a different nature (Strophogenesis), and to have arisen by the splitting of the genealogical individuals of a single generation into physiological individuals. Misled by the

[1] Haeckel.

[2] Strasburger (1).

differentiation of the moss-plant into axis and leaf, and by the view that sexuality must be associated with the more highly developed generation, the attempt is made to derive the fern-plant from the moss-plant alone, the sporogonium having no equivalent in the life history of the fern. This view lost sight of the homologies already recognised by Hofmeister throughout archegoniate plants. While the general statement, that the two generations in these plants arose by individualisation of parts of a single generation, appears to be in agreement with the antithetic theory, which has next to be discussed, the application of the view made in the comparison of Moss and Fern shows that it is essentially different.

The series of papers by Celakovsky and Pringsheim which have now to be mentioned are of special importance, since the problem of the nature of alternation in the archegoniatæ assumes in them the form which in the main holds at the present time. An important character, distinguishing these works from nearly all those previously referred to, is that alternation of generations in plants is considered by itself, no attempt being made to fit the phenomena into a classification of the analogous occurrences in animals. In a paper published in 1868,[1] Celakovsky recognises the work of Hofmeister and Pringsheim on the life histories of Algæ and Archegoniatæ as the basis for a consideration of the vegetable kingdom as a developmental whole. The sexual generation which is alone represented in most Algæ is termed the Protophyt, while the succeeding generation which produces reproductive cells asexually is the Antiphyt. This "polar alternation of generations" is distinguished from other subordinate forms. Among the Algæ it is found in *Œdogonium* and *Coleochæte*. In a subsequent paper[2] these views are elaborated, and a general survey taken of the phenomena included under the term alternation of generations. In accordance with the views of Braun, but in opposition to those already expressed by Sachs,[3] the shoot is recognised as the simplest vegetable individual, and

[1] Celakovsky (1). [2] *Ibid.* (2). [3] Sachs.

three forms of alternation of shoots are distinguished: (1) the alternating shoots owe their distinctness to phyllomorphism; (2) the leafy shoot is produced from a thallome, *e.g.*, moss-plant from protonema; (3) the second shoot generation comes into existence as the result of a sexual act (*Ascomycetes*, *Basidiomycetes*, *Florideæ*.) This third form is termed antithetic alternation of shoots. In contrast to alternation of shoots is placed the alternation of bionts; two essentially different kinds of this are distinguished as antithetic and homologous alternation. In the former, seen in Muscineæ and Vascular Cryptogams, two fundamentally distinct generations are present, the asexual either being immediately itself a fruit body, or as its ultimate aim producing fruits and spores. The existence of independent sexual and asexual plants, so commonly seen in Thallophytes, constitutes homologous alternation. If the alternation of asexual and sexual generations in Algæ be represented by AB, AB the antithetic alternation will be BC, BC. Among Algæ *Coleochæte* and *Oedogonium* possess the three generations A, B, C. The greater definiteness of the succession of the alternating antithetic generations is referred to the fact that the sexual cells and spores are strictly connected with their respective generations. "The protophyt is never able to produce spores as well as sexual cells, the antiphyt never sexual cells besides spores." In homologous alternation on the other hand the same individual may bear sexual and asexual reproductive organs. The difference is explained when it is remembered that the antiphyt was originally a fruit generation, and only later took on vegetative functions in the Vascular Cryptogams. In the concluding part of this paper, the views of Strasburger mentioned above are critically examined.

Alternation of generations was again considered by Braun in 1875.[1] The view taken agrees on the whole with that of Celakovsky, though the author inclines to regard the fruit generation, of *Coleochæte* at least, as homologous with the thallus of the sexual generation. He recognises the importance of antithetic alternation, al-

[1] Braun (3).

though he objects to the terms used by Celakovsky and employs instead embryonal alternation, distinguishing the generations as proembryonal and embryonal respectively. Antithetic alternation is considered to be peculiar to plants, a close analogy to alternation of generations in animals being found in alternation of shoots. The paper is of special interest from the broad philosophical standpoint assumed, which leads to the discussion of several factors in the problem, which have frequently been lost sight of. Thus Braun lays stress on the possibility that the development of a group of organisms may have been monophyletic or polyphyletic and gives point to the general discussion by suggesting that the Mosses are to be considered "as a further and, as it appears, geologically late development from Thallophytes." In discussing the phylogenetic significance of abnormalities, he points out that it is in most cases incorrect to assume that they are atavisms; they are rather of the nature of "morphogenetic possibilities". Lastly, this important paper deserves notice for the picture it presents of the state of "*Unsicherheit und Verwirrung*" in which the question of the nature of alternation had remained during the twenty years after the publication of the *Vergleichende Untersuchungen.*

In the year preceding this paper, the first case of the direct vegetative production of the fern plant from the sexual generation (apogamy) had been recorded,[1] and in 1876, Pringsheim,[2] after repeated trials, induced the corresponding phenomenon [3] of the vegetative production of the sexual from the tissues of the asexual generation in Mosses. These discoveries, the nature of which will be referred to later, led him to a theoretical view of the nature of alternation in archegoniate plants, which is essentially different from the antithetic theory. In the following year,[4] he developed this theory in detail. Two types of alternation in plants are recognised, (1) the vegetative alternation of shoots exhibited by both sexual and asexual generations, and (2)

[1] Farlow (1). [2] Pringsheim (4).

[3] This was afterwards termed "Apospory" by Vines (1).

[4] Pringsheim (5).

the sexual alternation of generations. The latter includes the admittedly homologous alternation in Thallophytes and the antithetic alternation in Mosses and higher plants. Between these, however, Pringsheim recognises a difference of degree only. He regards sporangia, and the sexual organs (antheridia and archegonia in the widest sense) as truly homologous structures, which have proceeded from one another, and considers that this relationship is made manifest "through the representative, correlative succession of generations with spores and of generations with sexual organs. The Thallophyta differ considerably in the relation borne by the sexual and asexual individuals to one another. In some, no alternation is found. The most common case is that a sexual generation alternates with a succession of neutral generations, the last of which again produces the the sexual form. The dimorphic character is, as a rule, exhibited only in the reproductive organs, the generations resembling one another in vegetative structure, and in the possession of subordinate forms of multiplication. In a number of cases, however, the first neutral generation differs more or less widely from those succeeding it. In several genera this generation is reduced to a sporangium, the spores from which may resemble in general appearance those derived from the neutral generation with developed vegetative parts. *Oedogonium Coleochæte* and *Cystopus* present in their fruit body, according to Pringsheim, such a reduced first neutral generation. In yet other algæ (*e.g.*, *Sphæroplea*), this is the only neutral generation found, and the alternation is a definite one between a single sexual and a single spore-bearing generation. The organisms, just mentioned, are compared by Pringsheim, as well as Celakovsky, with the simplest Bryophyte sporogonia. But the view, indicated above, of the phylogenetic history of the fruit body, of *Coleochæte* for example, leads to a conclusion as to the nature of alternation in the archegoniates essentially different from the antithetic theory. The phenomena of Apospory and Apogamy are considered by Pringsheim to support this conclusion that in the Bryophytes and Pteridophytes, we have simply a special case of homologous

alternation. The question of the mode of origin of the complicated and ultimately independent sporophyte of the latter group is not entered upon by Pringsheim.

The antithetic theory was restated[1] in the light of Pringsheim's views in 1877. Celakovsky recognises clearly the two main points contended for by that author, but while he admits that the fruit of *Florideæ*, *Ascomycetes* and *Hymenomycetes* does not represent the second generation (antiphyt), he maintains the view that the antithetic alternation in Mosses and Vascular Cryptogams is essentially distinct from the homologous alternation of Thallophytes. Certain additions to the antithetic theory must be mentioned, since they are the basis on which later work has in great part proceeded. The fruit body of *Coleochæte* is not recognised as a generation since all its cells produce swarm spores, but it is pointed out that from the *Coleochæte* fruit to the *Riccia* sporogonium is but a step. "This step is made in this way, an outermost layer of the spore-producing parenchyma transforms itself into a covering layer (wall of the sporogonium) and thus remains sterile instead of its cells changing to spores." The extension of this modification of spore-producing cells would lead to the kind of sporogonium found in the higher Bryophyta. Thus the Moss fruit is not homologous with the neutral generation of a Thallophyte, but is "a third newly arrived generation interpolated between the sexual and the first neutral generation". To illustrate this the series of forms *Vaucheria*, *Œdogonium*, and *Cystopus*, *Coleochæte*, and *Riccia* are used. Celakovsky does not admit the importance which Pringsheim attached to the phenomena of Apospory and Apogamy, as evidence of the homology of the sexual and spore-bearing generations. The former is to be explained by the origin of all the vegetative cells of the sporophyte from primitively reproductive cells, while Apogamy proves nothing further than that the second generation can arise from an indifferent cell of the prothallus, instead of from a special sexual cell by a sexual process. He points out that in these cases the alternation is not lost, the archegonial cell

[1] Celakovsky (3).

(ovum) does not give rise to a prothallus or the sporogonium stalk to a new sporogonium. The threefold alternation, which as the title of this paper indicates is recognised in the vegetable kingdom, may be mentioned in conclusion. The forms are: (1) Homologous alternation in the Thallophyta between two or more Protophyt generations. (2) Antithetic alternation between Protophyt and Antiphyt in the Mosses and Vascular Plants. (3) Homologous alternation between two or more Antiphyt generations, *i.e.* alternation of shoots in vascular plants, and especially in phanerogams.

Summing up the state of the question at this date (1878) we find that, owing in great part to the work of Pringsheim and Celakovsky, the morphological problem presented by the alternation of generations in archegoniate plants had been clearly recognised. It was seen that the true nature of alternation in Mosses and Ferns was only to be ascertained by arriving at correct views of the manner in which these groups had descended from lower forms such as those represented by the Thallophyta. On the one hand this might have taken place by the further development of a generation equivalent to the sexual generation, the individuals of which had become more or less reduced and remained in connection with the parent plants. On the other hand the elaboration of the product of fertilisation might have been an entirely new development, the result of which did not represent what had at any period been an independent generation. The further development of this interpolated generation with the appearance of vegetative organs might have proceeded by some of the spore-producing cells becoming sterile. On the first theory the parts of the second generation as well as the generation itself might exhibit homologies with the sexual generation on the latter homology (in the sense of homology by descent) is out of the question. It was as a subordinate extension of the homologous theory that Pringsheim[1] considered actual homology to exist between the seta of a moss capsule and the stem of the leafy plant.

Deferring until afterwards the consideration of theoretical

[1] Pringsheim (5).

views as to the nature of alternation which have been expressed since 1878, the results of a number of investigations, which have a more or less direct bearing on the question, may be first considered.

INVESTIGATIONS INTO THE NORMAL LIFE HISTORIES OF THALLOPHYTES AND ARCHEGONIATES.

The life histories of the main groups of Bryophyta and Pteridophyta, and of the Thallophytes which present the closest resemblance to them in this respect, were already so fully known that comparatively few facts of this kind bearing directly on the nature of alternation have been recorded since 1878. The important work of Klebs,[1] however, an account of which has already been given in SCIENCE PROGRESS,[2] has gone far to place the question of the nature of alternation in Thallophyta on an experimental basis. Klebs has shown that the alternation of the free living generations in a number of Algæ can be controlled by suitable modifications in the conditions of cultivation, and that, in *Œdogonium*, *Hydrodictyon*, and *Vaucheria* for example, the development of sexual or asexual organs of reproduction can be determined by the investigator. This places beyond any doubt the homology of the sexual and the free neutral generations in these Algæ, but it leaves untouched the all-important question whether or not the first neutral generation (to use Pringsheim's terminology) is or is not homologous with the other generations in the life cycle. To establish this homology, it will be necessary to show that the zygote of *Œdogonium* or *Coleochæte* can be induced to develop directly into the corresponding thallus, bearing sexual or asexual reproductive organs. The work of Klebs as yet published, although it does not settle this question, is of the highest importance, since it suggests the possibility of direct experimental evidence being obtained upon it in the Algæ at least. In this connection the work of Goebel,[3] Dodel Port,[4] and Klebs[5]

[1] Klebs (2). [2] Ward. [3] Goebel (3), (4).
[4] Dodel Port. [5] Klebs (1).

may be mentioned, since in it we find the first steps made in the experimental study of the gametophyte of mosses and ferns.

Treub's investigations[1] into the structure of the prothallus, and the development of the young plant in the genus *Lycopodium* have brought the close resemblance that may exist between the sporophyte and the prothallus of a Vascular Cryptogam into prominence. It is, however, a difficult question to determine how far morphological importance can be attached to the fact.

The work of Bower[2] on the normal development of Vascular Cryptogams, and especially of their spore-producing members has given considerable support to the antithetic theory, by showing that the structural facts in connection with the more recent modifications of these plants would bear the interpretation which that theory assumes. Omitting for the present the consideration of the theoretical views to which these investigations have led, the chief fact bearing on the question we are considering is that sterilisation of potentially spore-producing tissue has been shown to occur in the sporogonia of Bryophyta and in the sporangia of Vascular Cryptogams and Angiosperms. The complicated spore-producing structures seen in some of the latter groups may be viewed as having been derived from simpler sporangia in essentially the same way as the antithetic theory assumes the first stages of development of the sporophyte to have taken place. This view, originally suggested by Celakovsky, has been elaborated by Bower, and makes it possible to understand how the passage may have occurred from the wholly dependent Bryophyte sporogonium to the plant in the Vascular Cryptogams which is only at first dependent on the gametophyte. This comparison has been facilitated by fuller knowledge of the structure of the simpler sporophytes of the latter group especially that of *Phylloglossum*,[3] the full life history of which is unfortunately not yet known.

[1] Treub (1). [2] Bower (13). [3] *Ibid.* (2).

DEVIATIONS FROM THE NORMAL MODE OF ALTERNATION OF GENERATIONS.

The fact has already been mentioned that it was the discovery of apospory in mosses, together with the earlier discovery of apogamy in ferns, which led to the theory of homologous alternation being stated. Further investigation has strengthened the evidence to be derived from these deviations from the normal life history if—and this is still a disputed point—they afford any valid evidence at all.[1]

Little further has become known of apospory in mosses since Pringsheim[2] induced it in *Hypnum cupressiforme*, *H. Serpens* and *Bryum cæspitosum* and Stahl[3] confirmed his observations in *Ceratodon purpureus*. These experiments do not appear to have been further extended, but the interesting fact has been recorded[4] that a similar phenomenon may occur in sporogonia of *Funaria hygrometrica* while still attached to the moss plant, which was growing in a natural state. The sporogonia in question were found with atrophied capsules buried in the soil and giving rise to protonemal filaments. Such a case approaches closely to the hypothetical future development of a moss plant imagined by Pringsheim in 1877. No further investigations have been made to determine whether the gemma-like bodies found in capsules of *Eucamptodon perichætialis*[5] were aposporously developed as seems not improbable from the brief description.

The corresponding phenomenon in ferns, anticipated on theoretical grounds by Pringsheim,[6] has been made known and investigated in a number of species. Discovered by Druery[7] in *Athyrium Filix-fœmina* and by Wollaston[8] in *Polystichum angulare*, these early cases were fully investigated by Bower.[9] Apospory is now known in nine species, *viz* :—

[1] On the general question of the value of abnormalities in morphology, see Goebel (6), pp. 152 *et seq*.

[2] Pringsheim (4), (5). [3] Stahl. [4] Brizi.

[5] Montague. [6] Pringsheim (5). [7] Druery (1).

[8] Wollaston. [9] Bower (1), (3).

Onoclea sensibilis.[1]
Athyrium Filix-fœmina.[2]
Aspidium (Polystichum) angulare.[3]
Nephrodium Filix-mas.[4]
Scolopendrium vulgare.[5]
Pteris aquilina.[6]
Trichomanes alatum.[7]
T. pyxidiferum.[8]
T. Kaulfussii.[9]

It is unnecessary to give details of the phenomenon in the different species, but the general facts may be summarised. The prothalloid growths have been found to arise from the leaves or from young sporangia; in a number of cases their production is associated with a more or less complete sporal arrest. When the growth does not proceed from the sporangium it may occur from cells of the placenta, from the leaf-margin or from superficial cells of the leaf above the termination of a vascular bundle. Sometimes this commences while the leaves are still attached to the plant and standing erect; in other cases it has been induced by laying portions of the frond on damp soil. Special interest attaches to cases of apospory described in *Nephrodium Filix-mas*[10] and *Scolopendrium vulgare*[11] in which the prothalli arose from or near the margin of the early formed fronds of the sporophyte. Since fronds of this age do not in these species bear sporangia, sporal arrest can hardly have been a factor in the causation of the apospory. In *N. Filix-mas* the young plant was apogamously produced and the prothalli on its fronds were also apogamous.

Attempts to induce apospory by laying portions of fronds on damp moss have been made without success,[12] but indications of the effect of interference with normal conditions of life in at least predisposing to apospory are not wanting. Thus the prothalli of *Scolopendrium*, a number of which produced aposporous plants, had been repeatedly subdivided.[13] The *Onoclea* the aposporous growths formed upon sporophylls which had been induced to assume the characters of the sterile fronds by the removal of the latter from the plant. This may be regarded as an instance of experimental sporal arrest.[14]

[1] Atkinson (1), (2). [2] Druery (1), (4). [3] Wollaston; Bower (1).
[4] Druery (3), (4). [5] *Ibid.* (4); Lowe. [6] Farlow (2).
[7] Bower (5). [8] *Ibid.* (12). [9] Lowe. [10] Druery (3), (4),
[11] Lowe. [12] Bower (6). [13] Lowe. [14] Atkinson (2).

Apogamy which was discovered by Farlow in *Pteris cretica*[1] in 1874 and investigated in detail in this and two other species by De Bary[2] in 1878, is now known in a considerable number of species of Ferns. As in the case of apospory a list of these will be given and the general nature of the phenomena briefly considered.

Todea africana.[3]
T. rivularis.[4]
T. pellucida.[4]
Osmunda regalis.[5]
———
———
Ceratopteris thalictroides.[5]
Pteris cretica.[6]
(?) *Pt. serrulata.*[7]
Nephrodium Filix-mas.[8]
N. falcatum.[9]
N. dilatatum.[10]
N. Oreopteris.[10]
Aspidium aculeatum.[10]
A. angulare.[10]
A. frondosum.[10]
Doodya candata.[11]
D. Aspera.[16]
Athyrium Filix-fœmina.[12]
A. Niponicum.[10]
Scolopendrium vulgare.[10]
Notochlæna distans.[13]
———
Trichomanes alatum.[14]
———
(?) *Botrychium virginianum.*[15]

Apogamy may be defined, in the light of our knowledge of the cases in the above list, as the direct vegetative production from the prothallus of a complete sporophyte, or of any member or group of members of the latter, or of tissues characteristic of the sporophyte within the prothallus. In the first described cases a perfect sporophyte, which continued its growth in the normal manner, was formed on the under surface of a flat prothallus of the ordinary shape. There was more or less complete absence of archegonia from the prothallus in the substance of which tracheides were developed. A comparative review of the known cases shows however that they form a series as regards the directness of the origin of the sporophyte. It is convenient to describe the phenomena in this order, although it must be remembered that a single culture may show several of the forms here distinguished. Changes in the form of the

[1] Farlow (1). [2] De Bary. [3] Sadebeck. [4] Stange.
[5] Leitgeb. [6] Farlow (1); De Bary. [7] Trow.
[8] De Bary; Kny. Lang (2). [9] De Bary. [10] Lang.
[11] Stange; Heim. [12] Druery (5); Lang (2). [13] Berggren.
[14] Bower (5). [15] Jeffrey. [16] Mentioned by Sadebeck (2) p. 34.

prothallus usually occur first; it may become thick and fleshy and a cylindrical process may grow from the anterior margin or the under surface. Tracheides develop in the tissues of the prothallus before any other manifestation of apogamy. The apex of the cylindrical process may continue directly as the apex of a fern plant. In other cases isolated members of the sporophyte (ramenta, roots, leaves, sporangia[1]) may be developed from the process, which usually contains an axial strand of tracheides. In other cases (and sometimes in association with a cylindrical process) shorter conical projections develop from or around the sexual organs. One or many of these may give rise to sporophytes. All these peculiarities have been observed in prothalli, which would have produced normal embryos had fertilisation been permitted. This induced apogamy contrasts with the cases of direct apogamy in which the prothallus as soon as it has attained a certain size, and apparently independently of the conditions, is able to produce a sporophyte apogamously. Intermediate examples however occur which indicate that direct apogamy is only a special case of the capability which many normal prothalli show of producing the fern plant asexually. Apogamy may in seems be induced in many, possibly in nearly all, ferns by preventing fertilisation in prothalli growing under conditions favourable to nutrition. The cause of direct apogamy is still obscure.

With regard to early references to apogamy, the work of Wigand[2] must be mentioned. A careful study of his paper and figures has led the author to the conclusion that it is probable some of the prothalli he used in the course of his investigations were apogamous. In this fact the explanation of his opposition to Suminski's description of the functions of antheridia and archegonia may be found. The quotation from Wigand's paper given by Farlow[3] has, however, no reference to apogamy, but clearly relates to gametophytic budding. Mercklin's observation

[1] Sporangia developed from the prothallus are as yet known only in *Scotopendrium vulgare* and *Nephrodium dilatatum*.

[2] Wigand. [3] Farlow (1).

of tracheides in the prothallus may also be mentioned here.[1]

The relation which exists in some cases between apospory and apogamy on the one hand, and the much more common phenomena of sporophytic and gametophytic budding on the other deserves to be referred to in passing; further facts are required before any definite conclusions can be drawn. Certain examples of sporophytic budding have been regarded as extreme cases of apogamy resulting in the entire omission of the sexual generation from the life cycle.[2] In no case, however, does the evidence in favour of this interpretation seem to be sufficient. No really satisfactory cases of apogamy or apospory occurring in ferns in the natural state have yet been recorded. Apospory in *Pteris aquilina*,[3] on the prothalloid growths of which sexual organs were not observed, and the case of the occurrence of tracheides in a prothallus of *Botrychium virginianum*[4] were observed in wild plants. A number of aposporous and apogamous ferns are descended from wild finds, but in them it is uncertain whether the peculiarity has not been acquired under cultivation.

Treub[5] has recently recorded a case of apogamy in an Angiosperm (*Balanophora elongata* Bl.), the embryo of which develops without fertilisation from cells of the endosperm. The details of this interesting observation will be referred to below.

NUCLEAR DIFFERENCES BETWEEN THE ALTERNATING GENERATIONS.[6]

The recognition of a difference between the nuclei of the cells of the sexual and asexual generations constitutes

[1] Mercklin, p. 54. [2] Goebel (1). [3] Farlow (2).
[4] Jeffrey. [5] Treub (2).

[6] No attempt has been made to give a complete list of the literature relating to the reduction of chromosomes in plants, and only those papers which are necessary to illustrate the bearing of the general result on alternation are mentioned. Full references will be found in Strasburger (2) and (3) and in Zimmerman, *Morphologie u. Physiologie d. pflanzlichen Zellkernes*.

one of the most important additions to our knowledge of the facts of alternation. This is manifested by the number of chromosomes which are distinguishable in the dividing nucleus being twice as great in the cells of the sporophyte as in those of the gametophyte. This has now been established for representatives of the Bryophyta, Pteridophyta, Gymnosperms and Angiosperms, and, although the results need to be extended, may reasonably be assumed to hold throughout these groups. The increase in number of chromosomes takes place as the result of the sexual fusion; the double number is retained in the vegetative cells of the sporophyte; the reduction occurs in the spore mother cells. The existence of the double number of chromosomes in the cells of the sporophyte affords support to the antithetic theory of alternation, in that the spore-bearing generation appears as an interpolation, not merely between two successive gametophytes, but between the sexual fusion and the reduction in number of the chromosomes. The bearing of the facts known as to this periodic reduction of chromosomes upon the nature of alternation has been considered by Strasburger[1] in two important papers. His general conclusion may be given: "The morphological cause of the reduction in number of the chromosomes and of their equality in number in the sexual cells, is in my opinion phylogenetic. I look upon these facts as indicating a return to the original generation from which, after it had attained sexual differentiation, offspring was developed having a double number of chromosomes."

It thus becomes of great importance to ascertain whether corresponding phenomena take place in those Thallophyta which approach the Bryophyta most nearly in their alternation of generations. The facts are still unknown for such Algæ as *Oedogonium* and *Coleochæte* and in the only Alga yet accurately investigated (*Fucus*)[2] the reduction takes place just before the development of the sexual cells. *Fucus*, however, cannot be regarded as bearing directly on

[1] Strasburger (2), (3). See also the earlier paper by Overton.

[2] Farmer and Williams. See also Strasburger, *Jahrb. Wiss. Bot.*, 1897, p. 351.

the question at issue. In *Cystopus*, however, the life history of which was regarded by Celakovsky as corresponding to antithetic alternation, the nuclei in the developing oospore appear to possess twice the number of chromosomes present in those of the vegetative thallus. Reduction apparently takes place just before the division of the thirty-two nuclei which proceed from the nucleus of the zygote. From these about four times as many zoospores are produced. The details of this last division are not known but the analogy which it presents to tetrad division is obvious.[1]

The recent discovery of a sexual nuclear fusion preceding the formation of the fruit body of Ascomycetes has raised anew the question whether these plants do not exhibit an antithetic alternation. This has been discussed by Harper[2] but a definite conclusion is prevented by the absence of the necessary observations on the number of chromosomes present in the nuclei of the hyphæ before fertilisation.

While these facts regarding normal alternation appear to be most readily explained on the antithetic theory, it must not be forgotten that the phenomena of apogamy and apospory show that the nuclear changes (which presumably occur in these developments also) are not necessarily associated with the sexual fusion or spore formation. Nothing is known of the behaviour of the nuclei in these cases and it is unnecessary to discuss the question on *a priori* grounds in this place since this has been fully done by Strasburger[3] and Bower.[4] From this standpoint the case of apogamy in *Balanophora elongata*[5] possesses especial interest. The facts are briefly these. As the result of division of the primary nucleus, four nuclei are present at each end of the embryo sac. Three of these at one end belong to the ovum and the two synergidæ. The entire egg-apparatus and the four nuclei at the antipodal end of the embryo sac abort, taking no further part in the development. This proceeds from the polar nucleus at the egg-

[1] Wager, Berlese. [2] Harper. [3] Strasburger (2).
[4] Bower (11). [5] Treub (2).

apparatus end, which undergoes division *without previous fusion with the corresponding nucleus.* Within the prothallus which thus results the embryo is developed apogamously from a cell which occupies a certain position. It is of considerable importance as bearing on the nuclear changes which precede apogamy that the fusion of the polar nuclei (which in other cases is known to result in the doubling of the number of chromosomes in the nuclei of the endosperm) does not take place. The exact determination of the nuclear changes in this or similar cases would be of great interest.

THE DEVELOPMENT OF THE THEORIES OF ANTITHETIC AND HOMOLOGOUS ALTERNATION.

The nature of the alternation in Thallophytes was discussed by Vines[1] in 1879. His conclusion was that it is impossible to detect any distinct alternation in any but the *Coleochæteæ* and *Characeæ.* In the latter the pro-embryo was regarded as the sporophyte, the main shoot representing an aposporously produced oophyte.[2]

The views of Naegeli [3] differed from those of both Celakovsky and Pringsheim as to the divisions of the life cycle of the Thallophyta which are to be regarded as generations, and in the comparisons instituted between these and the two generations of Archegoniate plants. A full discussion is impossible, but the views expressed with regard to the alternation in *Ulothrix* or *Œdogonium* on the one hand and the Moss or Fern on the other will render the main point clear. The *Ulothrix* or *Œdogonium* plants reproducing asexually by zoospores are the repetition generations ($B_1 - B_n$); they are followed by one composed of individuals in the main similar to them, but bearing the sexual organs

[1] Vines (2).

[2] Vines (1), (2), (3). In connection with this view the fact that a reduction of chromosomes does not precede the development of the spermatozoids may be mentioned (Debski). Compare also Strasburger, (3), p. 413.

[3] Naegeli.

(the sex-producing generation C). The male and female reproductive cells and the zygote resulting from their union constitute the androgynous generation (D). This after a period of rest produces swarm spores, each of which develops into and represents the first stage of an individual of the sexually produced generation (A). The alternation is thus expressed

$$A.\ B_1-B_n.\ C.\ D.$$

The sexual generation of the moss (protonema and moss plant) is regarded as corresponding to the repetition generations and the sex-producing generation in *Ulothrix;* the sporophyte to the androgynous generation and the sexually produced generation. Using the same symbols as before, the life history of the moss is represented by

$$(B_1-B_n+C)\qquad(D+A).$$

While both in the antithetic and homologous theories the correspondence between the group of spores formed from the zygote in *Ulothrix* or *Œdogonium* and the moss sporogonium is recognised, on Naegeli's view the result of division of the zygote in these Algæ is not regarded as a generation. This introduces confusion into the comparison of the life history of the green Algæ with that of archegoniate plants. The application of these views to the problem of the probable course of evolution followed in the origin of Bryophyta and Pteridophyta from Algal ancestors appears to accord rather with the antithetic theory, though, as the comparison of the life histories given above show, points in common with the homologous theory also exist.

Some of the facts bearing on the nature of alternation which have been investigated by Bower have already been referred to; it remains, however, to indicate the main lines on which his elaboration of the antithetic theory has proceeded, and the views as to the phylogenetic relations existing between Algæ, Bryophytes and Pteridophytes expressed by him.[1] Agreeing with the distinction drawn by Celakovsky between antithetic and homologous alternation, and regarding the former as having originated by the inter-

[1] Bower (4), (7), (8), (9), (10), (13).

polation of a stage in the life history, Bower has traced the probable course of evolution of the sporophyte as illustrated by such a series as *Œdogonium*, *Coleochæte*, *Riccia*. But, going farther than Celakovsky, he has shown strong reasons for considering the increase in the complexity of the Bryophyte sporogonium to have been due to progressive sterilisation, and has extended this comparison to the lower Pteridophyta. The point of contact with ancestral forms, the sporophyte of which was of similar construction to a Bryophyte sporogonium, is sought among strobiloid types,[1] such as Lycopodineæ and Equisetineæ, and the working hypothesis is put forward "that in the strobiloid Pteridophyta the apex of the sporogonium is the correlative of the apex of the strobilus". In the transition from the sporogonial to the strobiloid form progressive sterilisation with differentiation of the sterile tissue, the formation of appendicular organs, and the subdivisions of the archesporial layer to form isolated patches instead of one continuous tissue are assumed to have taken place. In addition Bower has indicated the relation borne by these changes to the alteration in mode of life, which is assumed to have taken place on the spread of plants to the land, and has thus brought the biological aspect of the subject into proper prominence.

The relation of the course of evolution to the probable conditions is also dealt with by Atkinson,[2] who makes the additional suggestion that the disturbance of the assimilatory function of the gametophyte, induced by its spread to the land, would not only assist the sterilisation of some of the sporogenous tissue, but would tend to force the function of assimilation upon some of the sterilised regions of the sporophyte.

The views of Strasburger have been referred to above, and is is sufficient to say that on the ground of the facts known as to the periodic reduction of chromosomes he arrives at conclusions which are in essential agreement

[1] Goebel, on the other hand, has compared Mosses directly with the Ferns on the basis of resemblances in the sexual generation. Goebel (5).

[2] Atkinson (2), (3).

with those previously stated by Celakovsky and Bower as the result of morphological comparison.[1]

The nature of alternation has also been considered by Macmillan[2] and Fry,[3] but a consideration of these and other views would not aid in the elucidation of the main problem, since they differ mainly on points of detail.

Scott[4] has given a most suggestive review of the two alternative theories of alternation in his presidential address to the Botanical Section of the British Association. The critical nature of this work necessitates a reference to the original, but the important bearing of his clear restatement of the homologous theory upon the present state of opinion on the nature of alternation must be pointed out.

CONCLUSION.

To attempt a general discussion of the nature of alternation of generations and its relation to the successions of forms which have been distinguished as generations among the Thallophyta is not within the scope of this article. Some of the factors in the problem will, however, be referred to, and some lines of investigation, the results of which may be expected to aid in its solution, indicated in conclusion.

A study of the literature has made it evident that the facts have been interpreted very differently by the investigators who have discovered and discussed them. If we attempt to determine what is the essential distinction between the theories of homologous and antithetic alternation, the conclusion arrived at will, it appears to me, be of this nature. On the homologous theory, the sporophyte is to be traced back to a generation of originally independent individuals similar to those from which the gametophyte has arisen, the almost invariable alternation and the permanent or temporary dependence of the spore bearing on the sexual generation being subsequent adaptations. On the antithetic theory, the sporophyte is not derived from free-living in-

[1] Strasburger (2), (3).
[2] Macmillan.
[3] Fry.
[4] Scott.

dividuals of the ancestral algal form, but has had a distinct phylogenetic history as an interpolated stage in the life history. On the former view, the two generations of a moss are equivalent to two independent individuals of, *e.g.*, *Œdogonium*, on the latter to one individual and the zygote which produces the spores. If the first neutral generation of the ancestral form had, as Pringsheim's comparisons would suggest, become reduced to a group of spore-producing cells, the methods of advance in the complexity of the sporophyte need not have differed from those assumed by the advocates of the antithetic theory. But it is also possible that the differentiation of the two generations proceeded at first in free living individuals which only later became united to one another in almost invariable sequence. A provisional hypothesis as to how this might have occurred in such a group as the Ferns has been suggested by the author.

The views held as to the probability that one or the other course has been followed are intimately related to those on the connections by descent that exist between the different phyla of the vegetable kingdom. The usual view is that forms like the simpler liverworts descended from green algal forms which are represented by such existing Algæ as *Ulothrix*, *Œdogonium* and *Coleochæte*. From the simple sporophyte of these forms those of the more complex liverworts and the mosses on the one hand and of the ancestors of the vascular cryptogams on the other were evolved.[1] Even such a view involves the independent origin in the different groups of common characters of the sporophyte. But it is not inconsistent with any known facts to go a step farther, and to consider the possibility of a number of somewhat similar developments having taken place from the algal ancestry leading to various forms of simple sporophyte, some of which were physiologically independent after a time, while others were wholly dependent on the gametophyte. In particular, the possibility of the sporogonium of Bryophyta and the sporophyte in the

[1] Goebel while tracing the Pteridophyta to forms resembling the liverworts states clearly his opinion that their asexual generation proceeded on a different line from the commencement. Goebel (2), p. 401.

Vascular Cryptogams having arisen independently of one another must be considered. Such a view, while it does not prevent the use of the stages of complexity of the Bryophyte Sporogonium as illustrations of the probable steps by which the Pteridophyte Sporophyte was evolved, suggests the alternative course of looking for evidence which may indicate how the latter could have been derived directly from an algal form.

The modifications which the recognition of this possibility would entail in current views on homology cannot be touched upon here farther than to point out that the comparison of those forms of sporophyte which there are good grounds for concluding are homogenetic[1] with one another must be relied upon in preference to comparisons between forms which may be merely homoplastic.[1] General impressions, gathered from a wide survey of the relation of sexual and asexual generations in the vegetable kingdom, must be checked by comparisons limited as far as possible to closely related forms.

These considerations suggest further investigation of the behaviour of the sexual generation of the vascular plants in the hope that a more accurate knowledge of the changes which ensue on exposure to altered conditions of life may aid in arriving at conclusions as to how the sporophyte might have been evolved from organisms which in form, and possibly in physiological properties, resembled the gametophyte of existing plants. The changes of conditions to which most importance should be attached are such as may reasonably be supposed to have occurred during the evolution of land plants. From this point of view, considerable importance may be attached to apogamy, and in a less degree to apospory.

The existence of a nuclear distinction between the two generations is not necessarily inconsistent with such an origin of the sporophyte from forms homologous with the sexual generation. For it is an assumption that the nuclear

[1] Lankester. The extension of the use of these terms and of the distinction which they imply would do much toward clearing our ideas on many morphological questions.

difference which has been established is causally related to the origin of the second generation. It is quite possible that it may have been simply coincident with the germination of the zygote *in situ* without a previous rejuvenescence or division into swarm spores.

All progress in our knowledge of the relationships of the various groups of plants may aid in arriving at some determination as to the early course of evolution of the sporophyte. But in addition to the comparative morphology of existing forms, the experimental study of the Green Algæ, the Liverworts, and the simpler Vascular Cryptogams may be expected to yield important evidence on this question. The attempt to induce the zygote in *Œdogonium* or *Coleochæte* to develop directly into a sexual plant, and the experimental study of apogamy and apospory may be mentioned as bearing on the truth of the homologous theory. On the other hand, the experimental causation of sterilisation of spore-producing tissue of simple sporophytes by hindering the nutrition of the latter, as suggested by Atkinson,[1] might yield results confirmatory of the antithetic theory. Such induced changes, though merely "morphogenetic possibilities," would afford, if used with care, satisfactory guides to speculation. The fact that the changes are in many instances sudden and discontinuous need not in the light of recent work on variation exclude them from this use.

In concluding this outline of the history of the theory of alternation of generations in plants, hope may be expressed for a partial solution in the future, though this may be far distant. The clear recognition of the openness of the question is the best safeguard for the facts which support one or other theory having their proper weight accorded to them. The spirit in which such investigation should be carried on has been well expressed by Dr. Scott: "Let us in the presence of the greatest mystery in the morphology of plants keep an open mind, and not tie ourselves down to assumptions, though we may use them as working hypotheses".

[1] Atkinson (2), p. 180.

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THE FALL OF METEORITES IN ANCIENT AND MODERN TIMES.[1]

IN matters of scientific evidence relating to events which took place in early times nothing is more difficult than to place oneself in the position of a contemporary critic, amid the mental atmosphere of the time, and to regard the occurrence as it then appeared. One cannot help criticising it in the light of subsequent events, and early observers are, in consequence, too often condemned as credulous. In justice to our predecessors and to clear our own vision it is often profitable to review the development of some article of scientific belief, and to trace the steps by which it has been established.

In the case of meteorites and the belief in their fall from the sky, the story is a curious one, for this belief, though well founded and ultimately justified, for centuries met with opposition or disregard, not from ignorant people, but from the leaders of scientific thought.

The fairest, and doubtless the most interesting, way to gain a picture of the evidence available 100 years ago, of the impression which it produced upon thoughtful men, and of the reasoning by which they were ultimately converted, is to quote verbatim the vivid accounts of eye-witnesses, and the comments which they excited at the time.

The following fragmentary notes contain nothing new, except that some dispersed references are perhaps for the first time brought together.

By way of preface we may collect the main features of the evidence historical and contemporary as it presented itself to our ancestors towards the close of the last century.

Ancient literature, of course, abounds with references, some certain and some dubious, to the fall of stones from the sky; the great stones that fell from heaven in the battle of Gibeon, the hailstones and coals of fire of the eighteenth Psalm, are among the earliest; a Chinese account relating to the year 211 B.C. describes the fall of a star which

[1] A lecture delivered in Magdalen College, Oxford; 19th Feb., 1898.

turned to stone as it fell; and still earlier Chinese records go back to the date B.C. 644. In the Talmud is a legend concerning the plague of hail in Egypt, that the hailstones were very large, each of them being about the size of an infant's head; and that as they touched the ground they burst into flames. Livy mentions several instances of a rain of stones, and in the earliest reference which he makes, in his first book, to the shower of stones that fell about 652 B.C. on the Alban Mount he is careful to distinguish them from hailstones, "*haud aliter quam* quum grandimem venti glomeratam in terras agunt, crebri cecidere cœlo lapides".

The best established and the most famous of all in ancient times is that which fell about the time of the battle of Ægos Potami in B.C 403, and near the scene of the battle, as related by Plutarch in his life of Lysander.

Plutarch says that it was of great size and was held in great veneration by the people of the Chersonese who showed it in his own time. This fall is rendered doubly interesting by its association with the name of the philosopher Anaxagoras who is said to have foretold the event. On this subject Bayle in his Dictionary quotes Philostratus as attributing to Anaxagoras a great reputation for such predictions. At one time he predicted that on a certain day at noon the sun would become dark; at another he went to the Olympic Games with a cloak, knowing that it would rain, although the day was quite clear and serene; and a little while after it rained violently.

As is well known, the fall at Ægos Potami is still further confirmed by Pliny, who asserts that the prediction of Anaxagoras was made sixty-two years before the battle. He goes on to say: "The stone is still shown, of the size of a crowbar, and of a burnt colour. There was a comet at night at that time;" and further: "A stone is at the present day held in reverence at the school of Abydos; it is only small in size, but it is the one whose fall to the earth was foretold by Anaxagoras. It is also reverenced at Cassandria, now called Potidœa."

There can hardly be any doubt that, in spite of the legend about its prediction, all this refers to a real meteorite.

The criticism of Plutarch himself on the subject is interesting. He suggests that "shooting stars are really heavenly bodies which from some relaxation of the rapidity of their motion or by some irregular concussion are loosened, and fall not so much upon the habitable part of the earth as into the ocean, which is the reason that their substance is so seldom seen".

Aristotle in his chapter on meteors has some remarks on this event in which he seems to regard the stone as having been blown by the wind; but Plutarch, who discusses the theory held by some in his own time, according to which the stone was really torn by a hurricane from the top of a mountain, expressly rejects this theory.

Among these early accounts we find several accurate descriptions of all the phenomena which are now known to accompany the fall of a meteorite; the bright light, the noise of thunder or an explosion; and the stone itself is correctly described as of two kinds, either as a stony substance with a burnt black surface, or as metallic iron.

Thus in the chapter preceding that in which he describes the Ægos Potami *stone*, Pliny mentions the fall of a piece of *iron* among the Lucani in the year before Crassus was killed by the Parthians, and he describes this as being "spongiarum fere similis"; this expression at once recalls the aspect of several meteoric irons, notably that known as the Pallas iron which we shall have occasion to mention again.

It is indeed more than probable that most of the iron used by primitive people who have not learnt the art of treating iron ores was derived from such masses of meteoric iron; and it is to be noticed that in Siberia, Mexico, Chili and Arabia lumps of such material were not only used for weapons, but were much prized on account of their reputed heavenly origin; Barrow in his voyages reports a mass of this sort found in the mountains behind the Cape of Good Hope which was used in this way.

In this connection an interesting correspondence took place in 1870 between Sir John Herschel and the eminent Viennese mineralogist von Haidinger, relating to the epithet *αὐτοχόωνον* or "self-fused" applied to the iron quoit in the twenty-third book of the Iliad; the word is translated

"rudely cast" by Liddell and Scott, but it has been suggested that it means "native" as opposed to forged iron. Still more curious are two lines mentioned by Eustathius as interpolated near the opening of the fifteenth book of the Iliad relating to two μύδροι or "lumps" cast by Zeus upon Troy, ὄφρα πέλοιτο καὶ ἐσσομένοισι πυθέσθαι; and Eustathius adds: "Lumps of this kind are pointed out by the Periegetæ who call them anvils fallen from heaven".

In addition to the more or less direct evidence of which the preceding are examples there is abundance of indirect evidence derived from the worship of stones; for this worship must, I think, have at least sometimes originated in a meteoric fall.

Jevons in his *Introduction to the Study of Religion* traces the origin of stone-worship and of the anointing of stones merely to the veneration of those which had been used as altars, and this appears to be the opinion of most authors upon the subject. But, although it is by no means probable that most or even many of the holy stones were meteorites, it is more than probable that when so remarkable an event as the fall of a stone from the sky did take place it must have provoked religious awe, and the stone itself must generally have become an object of worship. It is certainly remarkable that this origin was ascribed to several of the holy stones of antiquity.

The Diana of the Ephesians of the Acts of the Apostles, the "image that fell down from Jupiter" is perhaps the best known instance.

The Caaba, or black stone of Mecca, venerated by all Mohammedans, was worshipped by the Arabians in very early ages, and, although it has not been seen by any one specially qualified to judge, is now generally supposed to have been meteoric in origin. In Sale's introduction to the Koran it is stated that this stone was supposed to have fallen down from heaven before the Deluge. Again Maximus Tyrius says that he had actually seen a quadrangular stone which was worshipped by the Arabians, and in the same passage he mentions that the Paphians worshipped a statue of Venus which looked like a white pyramid.

No doubt many of the holy stones were venerated on account of their form quite independently of their origin; the image of Venus in Cyprus is described by Tacitus as being not of human shape but conical; and he adds: "Et ratio in obscuro"; and Pausanias says that the images of Jupiter Melichius and of Diana were, the one a pyramid, and the other a column.

Even among the stones enumerated by Pliny which have been more or less identified with meteoric stones, the shape is one of the features according to which some at least were distinguished. The Ceraunia, or sky-stones, of his classification include as varieties stones which he refers to as Bætuli, Brontia and Notia, some of which have special shapes. All of these names frequently recur in mediæval literature.

It is evident that in one passage Pliny uses Ceraunia for a variety of precious stone, Beryl or Sapphire perhaps; but besides these he quotes Sotacus for the existence of two kinds of Ceraunia "which are black and red, resembling axes. Such as are black and round are holy things; cities and fleets can be captured by their means. A third sort greatly sought by the Magi are only found in places struck by lightning."

The word Bætylus remains a mystery; the name was primarily given to the stone which Saturn swallowed in mistake for Jove, but seems to have been subsequently applied to all meteoric stones. Hesychius suggests the Hebrew "Bethel" and the stone of Jacob as its origin; and this derivation seems to be accepted in the *Dictionary of the Bible*, though without any philological justification.

About the Brontia Pliny says that if we have sufficient faith we are to believe that they get into the heads of tortoises after thunderstorms; and here, I think, there is some confusion between the shape of some Brontia and the origin of others.

Through the midst of all this superstition, however, runs a continuous thread of reference to a celestial origin by which we are now able, in the light of subsequent experience, to trace a constantly recurring expression of the belief in meteoric falls.

The last statement for example in Pliny's enumeration appears to refer to meteorites; but the remark about axes may indicate that stone celts or hammer-heads are denoted by his first class. The Cambridge authority, King, compares the German word *Donnerkeil* for Thunderbolt; and again with the word *Bætuli* the Saxon "Beetle" which means a mallet, and concludes that these names in general refer to stone implements.

And here we are confronted by a curious complication in the history of the subject. Side by side with the fact that stones fell from the sky, existed the belief that the origin of a thing was indicated by its shape; consequently a celestial origin was ascribed to those stones whose shape resembled that of a missile, and both stony concretions, fossils such as echini, and stone celts were supposed to be meteorites. It is difficult now to disentangle the evidence of falls actually witnessed from that which is merely based upon the shape of the stones to which many of the mediæval accounts relate.

At the present day both Belemnites and the marcasite nodules found in the chalk are popularly supposed to be thunderbolts on account of their shape.

Conrad Gesner in his book *De Figuris Lapidum* (1565) describes the various stones which derive their names from their real or supposed meteoric origin, the Ceraunias, the Chelonitis, the Brontias, the Bætylus, and gives figures of many. Some of these are obviously fossils, others are stone implements; his accurate description of some which he had received as thunderstones from Kentman shows that they are clearly the latter. But it is equally certain that some of his words relate to real meteoric stones. He makes in particular this interesting remark: "The stone which fell from the sky in 1492 and is hung in the Church at Ensisheim and weighs 300 pounds (unless it has lost weight owing to the many visitors who take away fragments of it) has, I think, no particular shape"; and he mentions that he had actually received a piece of this stone.

So much for the general evidence available about 300 years ago; the last reference brings us to a time when stones

fell which are actually preserved at the present day, so that the veracity of contemporary accounts relating to them can no longer be questioned. From the sixteenth century onwards there are a number of such accounts in which we can now, reading by the light of subsequent experience, see internal evidence of their accuracy, and by which we are led to attach equal confidence to the accuracy of some of the earlier reports such as that of the Ægos Potami fall. Omitting, therefore, a number of mediæval references which may be found in the Saxon chronicles, Eusebius, Cardanus, Avicenna, Scaliger and others, we may pass directly to the Ensisheim fall, the earliest one of which we possess a contemporary account relating to a stone that still exists and has been proved to be meteoric.

Fall of the Ensisheim Stone.

The account is as follows :—

"On the 16th of November, 1492, a singular miracle took place. Between 11 and 12 in the forenoon with a loud crash of thunder and a prolonged noise heard afar off there fell in the town of Ensisheim a stone weighing 260 pounds. It was seen by a child to strike the ground in a field where it made a hole more than five feet deep. It was taken to the church as a miraculous object. The noise was heard so distinctly at Lucerne and many other places that in each of them it was thought that some houses had fallen. King Maximilian, who was then at Ensisheim, had the stone carried to the castle; after breaking off two pieces, one for the Duke of Austria and the other for himself, he forbade further damage, and ordered the stone to be suspended in the parish church."

With this may be compared an account quoted by Sir Norman Lockyer from a rare tract in the British Museum, in which the obviously truthful statement of the occurrence is somewhat obscured by the fancy begotten by terror.

The tract is entitled :—

Looke up and see wonders: a miraculous Apparition in the Ayre, lately seen in Barkeshire at Bawlkin Greene neare Hatford. And is as follows :—

"At Hatford some 8 m. from Oxford. Over this towne upon Wensday being the 9th of this instant Moneth of April, 1628, about 5 of the clocke in the after noone this miraculous, prodigious and fearefull handyworke of God was presented. A gentle gale of Wind then blowing from between the W. and N.W. in an instant was heard first a hideous rumbling in the Ayre, and presently after followed a strange and feare-full peal of Thunder running up and downe these parts of the countrey, but it strake with the loudest violence and more furious tearing of the Ayre about a place called the White Horse Hill. The whole order of this thunder carried a kind of majesticall state with it, for it maintayned (to the affrighted Beholder's seeming) the fashion of a fought Battaile. It began thus:—First for an onset went off one great Cannon as it were of thunder above like a warning peece to the rest that were to follow. Then a little while after was heard a second; and so by degrees a third untill the number of 20 was discharged in very good order though in very great terror. In some little distance of time after this was audibly heard the sound of a Drum beating a Retreate. Amongst all these angry peales shot off from Heaven this begat a wonderful admiration that at the end of the report of every cracke or Cannon-thundering, a hizzing noise made way through the ayre not unlike the flying of bullets from the mouthes of Great Ordnance; and by the judgment of all the terror stricken witnesses they were Thunder bolts. For one of them was seene by many people to fall at a place called Bawlkin Greene being a mile and a half from Hatford; which Thunder bolt was by one Mistris Greene caused to be digged out of the grounde she being an eye-witnesse amongst many other of the manner of falling. The form of the stone is three-square and picked in the end: The colour outwardly blackish, somewhat like Iron; crusted over with that blacknesse about the thicknesse of a shilling. Within it is a soft, of a gray colour, mixed with some kind of miner-all shining like small peeces of glasse."

With this may further be compared the record relating to a fall of iron at about the same date (1620) but in a very

different part of the world. The following is a translation by Colonel Kirkpatrick from a contemporary Persian account of which he possessed the manuscript written by the Emperor Jehangire himself.

Fall of a Persian Meteorite.

"Early on the 30th of Furverdeen, of the present year, and in the Eastern quarter of the heavens there arose in one of the villages of the Purgunnah of Jalindher, such a great and tremendous noise as had nearly, by its dreadful nature, deprived the inhabitants of the place of their senses. During this noise a luminous body was observed to fall from above on the earth, suggesting to the beholders the idea that the firmament was raining fire. In a short time the noise having subsided, and the inhabitants having recovered from their alarm, a courier was dispatched by them to Mahommed Syeed, the Aumil of the aforesaid Purgunnah, to advertise him of this event. The Aumil, instantly mounting his horse, proceeded to the spot where the luminous body had fallen. Here he perceived the earth, to the extent of ten or twelve guz in length and breadth, to be burnt to such a degree that not the least trace of verdure or a blade of grass remained; nor had the heat which had been communicated to it yet subsided entirely.

"Mahommed Syeed hereupon directed the aforesaid space of ground to be dug up; when, the deeper it was dug, the greater was the heat of it found to be. At length a lump of iron made its appearance, the heat of which was so violent that one might have supposed it to have been taken from a furnace. After some time it became cold; when the Aumil conveyed it to his own habitation, from whence he afterwards dispatched it in a sealed bag to court.

"Here I had this substance weighed in my presence. Its weight was 160 tolahs. I committed it to a skilful artisan, with orders to make of it a sabre, a knife, and a dagger. The workmen soon reported that the substance was *not malleable, but shivered into pieces under the hammer.* Upon this, I ordered it to be mixed with other iron.

"Conformably to my orders, three parts of the *iron of*

lightning were mixed with one part of common iron; and from the mixture were made two sabres, one knife and one dagger. By the addition of the common iron, the new substance acquired a fine temper; the blade fabricated from it proving as elastic as the most genuine blades of Ullmanny, and of the South, and bending, like them, without leaving any mark of the bend. I had them tried in my presence and found them cut excellently, as well indeed as the best genuine sabres. One of these blades I named *Katai* or *the cutter;* and the other *Burk-serisht* or *the lightning natured.*

"A poet composed and presented to me on this occasion the following tetrastich :—

This earth has attained order and regularity through the Emperor Jehangire :
In his time fell *raw* iron from lightning :
That iron was, by his world-subduing authority
Converted into a dagger, a knife, and two sabres."

With these early examples of the more modern and authentic records may be compared the two following which are quite modern : one relating to a meteoric stone that fell in Russia, and the other to an iron that fell in Mexico. The first has a special interest as the stone in which Diamond was found, and the second as the only modern meteorite which has been known to fall during a shower of shooting stars.

Fall of the Novo-Urei Stone.

"At 7·18 A.M. on 22nd of September, 1886, some peasants were working in a field at Novo-Urei in Russia.

"It was a dull morning without rain, although the sky was covered with clouds. Suddenly the air seemed filled with a bright light, followed in a few seconds by a violent report which was immediately succeeded by a second explosion. At the same moment the terrified peasants saw a fiery ball fall to the ground only a few yards from where they stood, and a second, but larger one was seen to descend into a neighbouring wood. The whole thing lasted less than a minute. The men fell in mortal terror to the ground and for some time dared not move. They thought that a frightful storm had burst over their heads,

and that fiery thunderbolts were falling. At length they recovered courage and went to the place where the thunderbolt had fallen. To their amazement they found here, in a small cavity a black stone half embedded in the earth, and still hot. It felt very heavy. They searched in vain for the other stone in the wood; but the next day a similar stone was found in a neighbouring field."

Fall of the Mazapil Iron, 1885.

"It was about nine in the evening when I went to the corral to feed the horses, when suddenly I heard a loud hissing noise exactly as though something red-hot was being plunged into cold water, and almost instantly there followed a somewhat loud thud. At once the corral was covered with a phosphorescent light, and suspended in the air were small luminous sparks as though from a rocket. I had not recovered from my surprise when I saw this luminous air disappear, and there remained on the ground only such a light as is made when a match is rubbed. A number of people from the neighbouring houses came running towards me, and they assisted me to quiet the horses which had become very much excited. We all asked each other what could be the matter, and we were afraid to walk to the corral for fear of being burned. When in a few moments we had recovered from our surprise we saw the phosphorescent light disappear, little by little, and when we had brought lights to look for the cause, we found a hole in the ground and in it a ball of fire. We retired to a distance fearing it would explode and harm us. Looking up to the sky we saw from time to time exhalations or stars which soon went out, but without noise. We returned after a little and found in a hole a hot stone which we could barely handle, which on the next day we saw looked like a piece of iron. All night it rained stars, but we saw none fall to the ground, as they seemed to be extinguished while still very high up."

But it is not necessary to multiply instances. It is clear that in ancient times and in the middle ages meteoric falls were often recorded and were implicitly believed by ordinary

people. Boetius de Boot in his book on Stones (1609) says: "Si quis hanc vulgi opinionem refellere velit insipiens videatur".

The preceding examples will serve as a sketch of the evidence which presented itself to scientific men in the last century.

Meanwhile, however—and this is the fact to which I wish particularly to draw attention because it makes the history of meteorites so curious as a study of scientific evidence—the whole subject had with the growth of scientific knowledge become gradually discredited among thoughtful and well-educated people. Now that we know the fact to have been true, it is easy on the one hand to make allowances for the fancy which enters so largely into such past accounts as that of the Hatford fall, and on the other to reject among the present records which appear from time to time in the public press those which describe the fall of stones during thunderstorms, and under other improbable or impossible conditions, as well as the details imputed by terror and superstition.

But before the fact was known to be true, the evidence was so vitiated by delusions of various sorts, and eye-witnesses were so apt to be deceived by the sudden nature of the event and the terror which it inspired, that those who were best able to criticise circumstantial evidence were the first to reject that relating to meteorites.

I rather suspect that this was also so among the ancients, although the same critical attitude towards such events would hardly be expected from them. Aristotle barely alludes to thunderstones; there appears to be no mention of them in Herodotus; and Lucretius only asks why a bolt never falls when the sky is unclouded.

In later times neither Locke, nor Bacon, nor Newton appears to make any reference to the matter; and Boyle only mentions meteorites as "Stones which pass among the vulgar for thunderstones".

At the end of the last century the leaders of scientific thought had criticised the evidence and rejected it *in toto*.

Their position is really very well expressed more than

a century before by Torbernus Bergmann, the celebrated Professor of Chemistry at Upsala, in his treatise *De Avertendo Fulmine* (1764), where he makes the following observation: "Popularis erat veterum Teutonum Suionumque opinio lapides quosdam de coelo mitti, quos Thors-vigger (Donnerkeile, *i.e.* Lapides Ceraunios s. Belemnitas) vocabant;" and then he states that three opinions concerning these Ceraunian stones are held among philosophers. (1) That the whole thing is a fable, and that the stones themselves are weapons in which the handiwork of man is clearly apparent; (2) that these stones really fell to the ground with the lightning, as is thought by the Arabians; in which case they may either have been carried into the air by the wind, or may have been generated in the air as is suggested by Cartesius; or (3) that they have been fused into a mass at the point where lightning has struck the ground; an argument adduced in favour of this view by Stahlius is that a certain man, expert in such matters, having found a little hole in the ground while he was digging predicted that there would be a ceraunian stone at the bottom; which proved to be the case.

Bergmann himself rejects the first two hypotheses as clearly absurd; but being convinced by the recent discoveries of Franklin that the phenomenon is electrical, thinks that the last explanation is not only possible but probable.

It is rather difficult now to realise the attitude of mind adopted by the leaders of thought at the beginning of the present century. There was no lack of evidence; plenty of witnesses asserted that they had seen the stones fall, and many of them were actually preserved. Shooting stars have of course always been familiar, just as they are at the present time, but the scientific authorities of that date after duly weighing the evidence came to the conclusion that there was no proof that these stars ever fell to the ground. They preferred to believe that those who professed to have witnessed such falls were mistaken, and that the supposed meteorites were ordinary stones struck by lightning. In fact, the witnesses generally mentioned thunder and lightning as accompanying the fall; this in itself was

suspicious; and, further, the witnesses were evidently so scared that they hardly knew what they had seen. And yet one cannot help feeling that the available evidence, if acutely criticised, was sufficient to enable a scientific critic to extract the truth from the mass of legend in which it was embedded; and in fact this was actually done with signal success by a writer whose work opens the last chapter in the history of the belief in meteorites.

The modern development of a scientific proof of the existence of sky-stones, as distinct from terrestrial material is no doubt familiar to many through Mr. Fletcher's admirable *Introduction to the Study of Meteorites.* At the risk of considerable repetition I must give a brief sketch of the meteoric events of the last decade of the eighteenth and the first decade of the nineteenth century, with the object of showing how the evidence was received by the critics of that date, and how they were finally persuaded. The chapter of proof really begins in the year 1794, when the German physicist Chladni wrote a very remarkable paper, "Ueber den Ursprung der von Pallas gefundenen und anderer ihr ähnlicher Eisenmassen".

The traveller Pallas in 1772 saw in Siberia a great mass of iron weighing about 1500 pounds which had been discovered by a Cossack at the top of a mountain near Krasnojarsk in Siberia. It was spoken of by the Tartars as a holy thing fallen from heaven. There was nothing like it in the neighbourhood and it was too large to have been transported to the mountain top by human agency. It was a peculiar spongy-looking mass which strongly recalls Pliny's description quoted above.

Chladni argued that this iron had evidently been fused, but not by man, electricity, or accidental fire, considering the place where it was found; there are no volcanoes anywhere in the neighbourhood; therefore it must have fallen from the sky. To the same origin he referred a huge mass found by Indians at Otumpa far away in the Argentine Desert of South America; a mass which was at first supposed to be an iron mine; and he suggested that other masses of native iron are also meteoric.

Chladni even went so far as to suggest that these masses were bodies of the same sort as those which produce the appearance of a shooting star in their passage through the air.

He subsequently fortified his views by an enumeration of a great number of reported falls of stone from the sky in ancient and mediæval times, of which I have quoted several above.

Of course Chladni's theory was not accepted—it was so improbable, and his arguments seemed to be only based upon the difficulty of accounting for the presence of this particular mass of iron in Siberia in any other way. His contemporaries regarded the essay as an ingenious but unconvincing piece of work.

Fall of the Sienna Stone.

Immediately after the appearance of Chladni's paper, however, a remarkable event took place at Sienna in Tuscany on 16th June, 1794, at 7 o'clock in the evening.

The event is thus described in the following letter from the Earl of Bristol to Sir William Hamilton which has been often quoted.

"In the midst of a most violent thunderstorm about a dozen stones of various weights and dimensions fell at the feet of different persons, men, women and children. The stones are of a quality not found in any part of the Siennese territory; they fell about eighteen hours after the enormous eruption of Mount Vesuvius; which circumstance leaves a choice of difficulties in the solution of this extraordinary phenomenon. Either these stones have been generated in this igneous mass of clouds which produced such unusual thunder; or—which is equally incredible—they were thrown from Vesuvius at a distance of at least 250 miles: judge then of its parabola. The philosophers here incline to the first solution. I wish much, sir, to know your sentiments. My first objection was to the fact itself, but of this there are so many eye-witnesses it seems impossible to withstand their evidence."

Sir Wm. Hamilton (*Phil. Trans.*, 85, p. 103), after quoting this letter says:—

"The outside of every stone that has been found, and

has been ascertained to have fallen from the cloud near Sienna, is evidently freshly vitrified, and is black, having every sign of having passed through an extreme heat; when broken, the inside is of a light grey colour mixed with black spots, and some shining particles, which the learned here have decided to be pyrites, and therefore it cannot be a lava, or they would have been decomposed. Stones of the same nature, at least as far as the eye can judge of them, are frequently found on Mount Vesuvius; and when I was on the mountain lately, I searched for such stones near the new mouths, but as the soil round them has been covered with a thick bed of fine ashes, whatever was thrown up during the force of the eruption lies buried under those ashes. Should we find similar stones with the same vitrified coat on them on Mount Vesuvius, as I told Lord Bristol in my answer to his letter, the question would be decided in favour of Vesuvius; unless it could be proved that there had been, about the time of the fall of these stones in the Sanese territory, some nearer opening of the earth, attended with an emission of volcanic matter, which might very well be, as the mountain of Radicofani, within fifty miles of Sienna, is certainly volcanic. I mentioned to his lordship another idea that struck me. As we have proofs during the late eruption of a quantity of ashes of Vesuvius having been carried to a greater distance than where the stones fell in the Sanese territory, and mixing with a stormy cloud have been collected together just as hailstones are sometimes into lumps of ice, in which shape they fall, and might not the exterior vitrification of those lumps of accumulated and hardened volcanic matter have been occasioned by the action of the electric fluid on them? The celebrated Father Ambrogio Soldoni, professor of mathematics in the university of Sienna, is printing there a dissertation upon this extraordinary phenomenon, wherein, as I have been assured, he has decided that those stones were generated in the air, independently of volcanic assistance."

Soldoni's account contains the following additional details: "Two ladies being at Coyone, about twenty miles from Sienna, saw a number of stones fall with a great noise

in a neighbouring meadow; one of which, being soon after taken up by a young woman, burnt her hand; another burnt a countryman's hat; and a third was said to strike off the branch of a mulberry tree, and to cause the tree to wither".

Soldoni himself thought that "the stones were generated in the air by a combination of mineral substances which had risen somewhere or other as exhalations from the earth, but not from Vesuvius".

Very shortly afterwards (1796) appeared the work of Edward King, *Remarks Concerning Stones said to have Fallen from the Clouds*, in which this and other falls were enumerated and discussed. In regard to the Sienna stones he recalls instances in which volcanic dust was known to fall upon ships 100 leagues from the scene of eruption, and quotes Sir William Hamilton's account of the Vesuvius eruption in which ashes appeared to be projected to a height of twenty-five or thirty miles; he suggests as an explanation of the Sienna stones that these ashes were carried beyond Sienna northwards, and were then brought back by a northerly wind, congealing from the air, which he had always regarded as "the great consolidating fluid out of which all solid bodies are composed".

Fall of the Wold Cottage Stones.

At the very time when King was writing, a stone was being exhibited in London which weighed fifty-six pounds and was seen to fall at Wold Cottage in Yorkshire on 13th December, 1795.

The following is the account given by the handbill which accompanied the exhibition: "It penetrated through twelve inches of soil and six inches of solid chalk rock, and in burying itself had thrown up an immense quantity of earth to a great distance; as it fell a number of explosions were heard about as loud as pistols.

"In the adjacent villages the sounds heard were taken for guns at sea; but at two adjoining villages were so distinct of something passing through the air towards the habitation of Mr. Topham that five or six people came up to see if anything extraordinary had happened to his house or

grounds. When the stone was extracted it was warm, smoked, and smelt very strong of sulphur. Its course, as far as could be collected from different accounts was from south-west. The day was mild and hazy; the sort of things very frequent in the Wold Hills where there are no winds or storms; but there was not any thunder or lightning the whole day. No such stone is known in the country. There was no eruption in the earth: and from its form it could not come from any building, and as the day was not tempestuous it did not seem possible that it could have been forced from any rocks, the nearest of which are those of Flamborough Head, a distance of twelve miles. The nearest volcano I believe to be Hecla in Iceland."

It might be thought that an examination of the stones themselves would be sufficient to prove or to disprove the common belief about their origin; and about this time an examination of the sort was undertaken by some of the leading French chemists, who actually made an analysis of the Ensisheim stone, and, finding it to contain nothing new, concluded that it was terrestrial. Their report on these supposed sky-stones terminated with the words: "Ignorance and superstition have attributed to them a miraculous existence at variance with the first notions of natural philosophy".

Fall of the Benares Stone.

In the year 1798, another well-authenticated fall took place in India, fourteen miles from Benares, where a luminous meteor was observed in the western heavens at 8 P.M. accompanied by a loud noise resembling thunder. The sky was perfectly serene; not the smallest vestige of a cloud had been seen for about eight days, nor were any seen for many days after. "Inhabitants observed that the light and thunder were accompanied by the noise of heavy bodies falling. Uncertain whether some of their deities might not have been concerned in this occurrence they did not venture out to inquire into it until the next morning, when the first circumstance which attracted their attention was the appearance of the earth being turned up

in different parts of their fields, where on examining they found the stones."

Again in the same year a fall was reported at Villefranche, near Lyons; the meteor was seen by many people and the eye-witnesses were horribly alarmed. One man whose house was within twenty paces of the spot where the stone fell was so terrified by the noise that he "shut himself up with his family in the cellar, and then in the bed-chamber, where, fear prevailing over curiosity, he spent the night without daring to go out to examine what had happened".

By this time Chladni's memoir had attracted attention to at any rate the possibility of the truth of such reports, and all these recent occurrences gave rise to much discussion. It will be sufficient to quote a few of the contemporary criticisms in order to gain some idea of the prevailing impression which they created among those who read them.

W. Beauford writing in the *Philosophical Magazine* in 1802, concludes that the matter must be of volcanic origin and derived either from Vesuvius, Etna, or Hecla. But the distances are too far for them to have traversed as stones. "Hence, if they originate from volcanic ashes they must be formed in the clouds where those ashes meeting with carbonic, sulphuric and other acids, and mixing with earthy particles drawn from terrestrial objects are by the electric fluid in the lightning precipitated from the aqueous vapours which bore them up, and, becoming united, fall to the earth in the form of stones, as in some measure is evinced from the flashes of light and detonation which accompany their fall."

Pictet writing on behalf of the French National Institute in 1803 expressed the opinion that "the attention of philosophers should be directed to the subject in order that the phenomenon if true may be confirmed—or if only an illusion supported by popular error may be consigned for ever to the class of errors". In the same year the French Institute mentions new motives to "induce philosophers to examine and appreciate the different testimonies in consequence of which the stones in question have been supposed to have fallen from the clouds. When a phenomenon

is announced if we were able to ascertain by a complete enumeration of the different physical agents that none of them is capable of producing it the impossibility of the phenomenon would be the inevitable result and consequently the falsity of the account. But on the other hand, when we find a cause which establishes the possibility of it, if sound logic forbids us to ascribe it exclusively to this cause, it commands us at the same time to substitute doubt for complete negation and to employ every means possible of confirming the fact, because it is not repugnant to the general laws of Nature."

This very guarded and somewhat curious statement is explained by the fact that Laplace and Poisson had calculated that a body projected from the moon would require only a velocity five times as great as that of a bullet of a twenty-four pounder, discharged with a quantity of gunpowder equal to half its own weight, to reach the earth after a journey of sixty-four hours, and would arrive with a velocity of 31,000 feet a second. It is evident that the accounts of the falls themselves were by this time no longer discredited, and that even the lightning theory was losing its adherents.

In 1803 Olbers, who had at first asserted that the Sienna stones were from Vesuvius, is led by the similarity of the sky-stones in different parts of the world to agree that they had a common origin and probably came from the moon. The chemist Vauquelin also inclined to the moon theory ; it is evident that the absence of atmosphere there would account for the stones leaving a lunar volcano without retardation and and also without experiencing oxidation. Writing of the Barbotan fall which took place in 1789 he says : " Some peasants brought stones which they said were the result of the fall of the meteor ; but at that period they were laughed at. What they said was considered as fables—and those to whom the stones were offered would not accept of them. The peasants would now have more reason to laugh at the philosophers."

Even at this period, however, when it began to be suspected that stones really fell from the sky and that they may have a common origin, it was by no means universally conceded that they were extraterrestrial.

Proust, in a paper published in the *Journal de Physique* in 1805 (reported in *Nicholson's Journal*, vol. xii.), describes a stone which fell in 1773 at Sena in the district of Sigena, in Spain; and gives the results of an analysis. He concludes that such stones "cannot subsist in any of the habitable parts of the globe. But from the eternal cold of the polar regions, where water remains for ever a solid mass, and iron cannot rust, we may reasonably look to these regions as the native place of such bodies."

But we can now hurry to the close of the story.

It is pretty evident from the preceding quotations that at the beginning of the present century the attitude of scientific men towards the reported fall of meteorites was one of suspicious indifference. There might be something in it all; there was fair evidence in many cases that something startling had happened; but no reliance could be placed upon the evidence of the senses under such conditions; and the witnesses were generally ignorant rustics.

It had been proved by Franklin that lightning is the same as the electric spark; and thunder is an accompaniment of lightning. The witnesses of these events professed to have heard thunder; what they saw and found were, no doubt, ordinary stones struck by lightning; and this conclusion seemed to be supported by chemical and mineralogical study of the stones themselves.

In the meantime an English chemist was, unnoticed, pursuing the only satisfactory method of completing the scientific proof which had been initiated by Chladni's acute reasoning.

This chemist, Edward Howard by name, collected pieces of four stones, those which fell at Sienna, Wold Cottage, Benares, and one which fell during a thunder-storm in 1753 in Bohemia. He made analyses of them and submitted them for mineralogical investigation to the Count de Bournon.

The results of his long and patient investigation were communicated to the Royal Society in 1803. He concluded that all these four stones had nearly the same chemical composition; and that though there was nothing actually new in them, their mineral composition was so unlike that of all terrestrial stones, and so similar for the four

masses—though they came from widely distant places and were asserted to have fallen at very different dates—that they must have had a common origin ; and he concluded, though with diffidence, that they may very possibly be really meteoric.

This paper attracted much attention in the scientific world, and the opportunity for putting it to the test soon occurred in France, where the new views met with the greatest opposition. A shower of stones fell on 26th April, 1803, at L'Aigle in the department of Orne. The eminent physicist Biot was sent down by the French Academy to investigate the matter, and reported that there was no doubt that a violent explosion was heard that day for seventy-five miles round ; that a fire ball was seen, though the sky was clear ; and that about 3000 stones fell within a space of six by two miles.

From this time the fall of meteorites was no longer doubted. The subsequent discoveries and the present state of our knowledge are admirably stated in Fletcher's *Introduction* referred to above, and can be further pursued in the special treatises on the subject.

On a review of the whole story one cannot help feeling that although the scientific proof could never have been complete without the work of Howard, and that his work was of an extraordinarily difficult nature, as is proved by its previous failure in the hands of the French chemists, yet the arguments of Chladni might have been advanced at almost any previous period had some sufficiently acute critic cared to examine the evidence without prejudice. The history traced in the foregoing pages is a curious study of the rejection of circumstantial evidence owing to its surprising nature and to the superstition with which it was mixed. The fault lay, as is clear from the official statement of the French Institute, in the refusal to accept the evidence relating to a phenomenon for which a sufficient cause could not be at once suggested—a very common but a very dangerous attitude. Doubtless our successors will be able to regard with equal curiosity either the prejudice or the credulity with which many a problem is regarded at the present day.

H. A. Miers.

METABOLISM OF THE SALMON.

"THE curious life history of the salmon has always been a subject of the deepest interest not only to the zoologist and physiologist, but also to the sportsman. In spite of the most careful study by scientific investigators, the migrations of the salmon and the various changes in condition that it undergoes are even now far from being fully understood; and the careless observations and foolish traditions of keepers, fishermen and gillies have only served to involve the matter in a deeper cloud of mystery."

The foregoing is the opening sentence of a report[1] which has recently been presented to the Fishery Board for Scotland by Dr. Noël Paton. In order to dispel this veil of ignorance, Dr. Paton has undertaken an extended series of observations, which have been carried out in the Research Laboratory of the Royal College of Physicians of Edinburgh. The subdivisions of this subject are so numerous, and the points to be investigated so diverse that Dr. Paton has adopted the wise measure of obtaining the co-operation of several other workers in the laboratory. This union of forces has produced a result which would have been beyond the power of any individual investigator.

The principal subjects of the research consist in a verification of the alleged abstention from food which the fish exercises when in fresh water, the details concerning the growth of the generative organs which occurs during this period, the simultaneous decrease in the muscular tissue, and the consequent deterioration of the food value of the fish, a discussion on the sources of muscular energy, and of the metabolic exchanges in fats, proteids, iron, phosphorus, pigments and so forth.

Such an enumeration of the chief subjects treated will indicate the wide scope of the work; in fact it forms the

[1] *Report of Investigations on the Life History of the Salmon*, edited by D. Noël Paton, 1898. See also *Journal of Physiology*, 1898, vol. xxii., p. 333.

most important contribution to the subject which has appeared since the publication of Miescher-Ruesch's writings. Some of the outcomes of Miescher's work have already appeared in this Journal[1] in papers by Dr. Brodie and Mr. Escombe, though neither of these papers was written with special reference to the salmon.

Miescher's observations were made on Rhine salmon, and the principal conclusions he drew were that the fish does not feed during its sojourn in fresh water, that the fat and proteid stored in the muscles is transferred to the growing ovaries and testes, but that the material stored in the muscular tissue is more than sufficient for that purpose.

It was to fill up numerous gaps in these observations that Dr. Paton undertook the large task he has so successfully completed, and every page of his full report is worth the careful study of physiologists. All I am able to attempt here is a brief summary of his principal conclusions.

Before doing this, however, let me enumerate some of the subjects of biological and economic interest which were not investigated. This I may best do by another quotation from the concluding sentences of the report: "As regards the course of migration, our investigations cover only a few months of the year, and interesting results are to be expected by extending the investigations into other seasons. Whether the *rate of migration* can be satisfactorily ascertained in our short Scottish rivers is very doubtful. In the great Canadian rivers, such as the Fraser, very valuable results might be expected from a study of this question. Indeed it would be a matter of the greatest importance to have the observations recorded in these papers checked and extended on a large scale in such a river, with its unbounded supply of fish and hundreds of miles of waterway. . . .

"The downward migration of kelts (the young fish) requires further study. Of the twenty-two kelts received in April, 1897, all were females. Is this a mere accident,

[1] SCIENCE PROGRESS, April, 1898, "The Phosphorus Containing Substances of the Cell," by T. G. Brodie; "Germination of Seeds," by F. Escombe.

or do the male kelts descend at a different time? The interesting question of the loss of the great maxillary development in the male is also yet to be elucidated."

May I be permitted to re-echo this hint to our Canadian cousins? Those of us who last year had the opportunity of experiencing Canadian hospitality will know the favour with which science is regarded in the Dominion by people and Government alike; and of the many wonderful sights those who travelled to the West were privileged to witness, that of the salmon in the rivers of British Columbia will probably be the one which will most vividly imprint itself on the memory. We hope that some scientific use may be made of such opportunities.

Turning, however, to the body of the report, let me now briefly indicate the principal lines of research contained in it.

The first question: Do salmon feed while in fresh water? was taken up by Drs. Gulland and Gillespie. It seemed probable that a conclusive answer might be arrived at by three different lines of investigation, *viz.* :—

(1) The condition of the mucous membrane of the alimentary canal.

(2) The activity of the digestive secretions.

(3) The bacteriology of the alimentary tract.

In connection with the first of these questions, the inactive condition of the glandular epithelium, which was in great measure desquamating, the absence of zymogen granules in the pancreas, the fatty condition of the liver, the emptiness of the gall bladder, and the absence of even a trace of food, all point conclusively to the inactivity of the alimentary canal. This conclusion is supported by what was found in connection with the second question, namely, that the proteolytic and diastatic action of extracts of various parts was extremely low. If, for instance, in the case of the stomach, the peptic activity be expressed in the case of kelts as 30, that of fish from the estuaries was 9·5, and from the upper waters, 9·1. The digestive activity was proportional to the acidity of the glycerine extract, and this in turn was in inverse proportion to the number of micro-

organisms. The acid present is organic in nature, and hydrochloric acid is absent. The number of bacteria in the tract is very great, especially during the warm summer months; this fact is opposed to Miescher's idea that putrefaction does not occur so readily in the upper water fish as in those from the estuaries. The increase in organisms is probably due to the diminished acidity of the gastric contents.

In making the observations bearing on metabolism, Dr. Paton is himself principally responsible, though some of the details were left to his colleagues, Dr. J. C. Dunlop, Mr. Mahalanobis, Dr. F. D. Boyd and Dr. E. D. W. Greig.

Salmon were procured from the estuaries and from the upper reaches of the three rivers, Helmsdale, Spey and Dee, from May to November. It was not possible to say when the fish captured in the upper waters in May and June had left the sea, and it was considered fairest to limit the investigation to a comparison of the upper water fish of July and August onwards with the estuary fish from May onwards. The whole fish in each case was available for analysis; in the muscles, separate analyses were made of both "thick" and "thin" portions of the musculature, and for tabular purposes, the figures are in all cases reduced to a fish of standard length, *viz.*, 100 cm., the length of a salmon of about 30 lb.

The tables of analyses given bear witness to the loss of total solids in the muscles, and the gain in the weight of ovaries or testes as the case might be. The large difference between these two numbers gives the amount available as a source of energy.

Among the solids particular attention was directed first to the fats, and secondly to the proteids. The fat stored round the pyloric appendages and in the liver is also taken into account. The numbers are much more complete with regard to female fish than male fish, the number of male fish available being too small for the drawing of satisfactory averages.

The changes in the muscle are described by Miescher-

Ruesch as a degeneration, but this conception of the process is not supported by histological examination. What occurs is an accumulation of fat outside and within the fibres while the fish is feeding, and this is subsequently, during the inanition period, used up for the construction of the genitalia and as a source of muscular energy.

The "curd" of salmon muscle which is so marked in the fish just leaving the sea early in the year is composed of proteids and fats.

The nature of the proteids in the muscles was investigated by Dr. Dunlop. He finds paramyosinogen, myosinogen, and myoglobulin which are all soluble in salt solution; a nucleo-proteid corresponding to that found by Pekelharing, and called myostromin by Karajew; this is partly soluble in salt solution, and wholly so in 1 per cent. caustic soda solution. Collagen from the interstitial connective tissue was not dissolved by either reagent. True albumin was not found, and proteoses and peptose were also absent. No mention is made of what v. Fürth calls myo-proteid, and which he considers to be characteristic of fishes muscle, nor of Siegfried's nucleon. It is apparently the soluble proteids which undergo the diminution demonstrated to occur as the fish proceeds to the upper waters.

Coming now to the summary of metabolic exchanges in fat and proteid, we enter the region of controversy; and in view of the recent discussion by Pflüger and others of the part played by fats and proteids respectively as a source of muscular energy, the present observations on a cold-blooded animal in which the decomposition of the fats can be only sparingly connected with the evolution of heat, are of no little interest.

If the fat and proteid going to the ovaries and testes is subtracted from that lost from the muscles, the residue gives the amount available for the liberation of energy. The absolute numbers are given in the report, but it will be sufficient here to mention the proportion found. In female fish up to August the energy from proteids to energy from fats is as 1 : 4·2; extending the observations to November the proportion rises to 1 : 7·6. In male fish

up to August, the proportion is 1 : 11·6. Sufficient male fish in the later months were not obtainable.

In connection with phosphorus, the results indicate that the phosphorus stored in the muscles as simple phosphates is transferred to the ovaries and testes, and there built up into organic combinations. In both glands, lecithin appears to occupy an important step in this conversion; but while in the testes the change to the true nucleins is carried out at once, in the ovum an intermediate product ichthulin (a pseudo-nuclein) is first formed, and undergoes the change into nuclein as the embryo develops.

The gain of iron which occurs in the ovaries is not derived from the muscles nor from the liver. In all probability its source is therefore the hæmoglobin of the blood.

The last point I shall mention is that relating to the pigments, which were investigated by Miss M. I. Newbigin, who is already well known from her work on Crustacean pigments. The colour of salmon flesh is due to two lipochromes, one being the widely distributed yellow pigment lutein, the other a bright red pigment closely resembling that occurring in the Crustacea.

The same pigments are found in the ovaries, and as the season advances the red colouring matter accumulates in the ovaries and disappears from the muscles. The red lipochrome probably originates from the yellow, and the latter is probably derived from the herring, etc., on which the salmon feeds. The purpose which the pigment seems to serve is to assist in concealing the ova when they are shed.

The main conclusions to which the report points are the following: First, that when the fish enters the river it ceases to feed, and has to rely entirely on its own resources, namely, the materials stored in its muscular tissue. The evidence on which this rests is much more conclusive than in Miescher's earlier investigations; though perhaps the sceptical fisherman may still ask, Why, then, does the salmon rise to the fly?

Secondly, of the stored material by far the larger fraction is used for the development of kinetic energy; this is especially true for the fats, particularly in the latter portion

of the fish's stay in fresh water. A comparatively small fraction of the muscular store is transferred to the growing genitalia. The substances thus transferred are proteid, fat, phosphorus-containing materials, and pigment; the phosphorus containing materials undergo certain chemical changes in the transference, but serve chiefly in the synthesis of nuclein; it appears probable that the proteid and fat would undergo analogous intramolecular rearrangements also. Of the substances under investigation only one, the iron, is not derived from the muscle; this comes from the hæmoglobin of the blood.

W. D. HALLIBURTON.

THE PHYSIOLOGICAL EVOLUTION OF THE WARM-BLOODED ANIMAL.

IN physiological text-books it is customary to point out how widely warm- and cold-blooded animals differ from each other in the absolute temperature of their bodies, and in the manner of their reaction to change of external temperature. In practically no case, however, is an attempt made to connect the two classes of animals, and to show, by means of connecting links, how the one class may have been evolved from the other. Doubtless this was in part due to actual lack of data. But now, in consequence of the recent publication of a very interesting paper by Sutherland,[1] on the temperatures of monotremes and marsupials, it is possible to trace with more or less completeness the various stages by means of which the lower invertebrates may have been gradually evolved, in a physiological sense, to produce a warm-blooded animal, such as man, in which the nervous system appears to possess almost perfect power of keeping the temperature of the body constant, whatever be the temperature, or variations in the temperature, of the immediate environment.

It might be thought that, in their reaction to temperature, all cold-blooded animals are alike. But this is by no means the case. There has taken place among them a gradual evolution of the nervous control of the tissue metabolism, which is probably quite as great as that separating the typical warm-blooded animal from the higher cold-blooded one. There would even seem to be a gradual evolution in the reaction of tissue change to temperature in respect of the tissues themselves, apart from a special nervous controlling influence. Thus the writer,[2] as the result of observations on the respiratory activity of various marine invertebrate and vertebrate animals, at various temperatures, came to the conclusion that this was not by

[1] *Proc. Roy. Soc. Victoria*, vol. ix., p. 57, 1897.
[2] *J. Physiol.*, xix., p. 18, 1896.

any means equally affected in the different animals by equal variations of temperature. In the following table are given what were called the "temperature increments" of the animals experimented on, or the relations of the respiratory activity at 24° to the respiratory activity at 10°:—

Name of Animal.	Class or Order.	Temperature Increment.	Per cent. Solids in Tissues.
Beroe ovata	Ctenophora	5·1	·60
Cestus veneris	„	4·4	·24
		mean 4·8	mean ·42
Salpa tilesii	Tunicata	4·5	·43
Salpa pinnata	„	4·3	·26
		„ 4·4	„ ·35
Rhizostoma pulmo	Scyphozoa	3·7	·53
Carmarina hastata	Hydrozoa	2·2	·38
		„ 3·0	„ ·46
Pterotrachea coronata	Hetoropoda	3·2	·53
Octopus vulgaris	Cephalopoda	2·5	11·7
Tethys laporina	Gasteropoda	2·0	1·20
		„ 2·6	„ 4·48
Amphioxus lanceolatus	Acraniata	2·7	12·8
		„ 2·7	„ 12·8
Serranus scriba	Pisces	2·6	16·7
Heliases chromis	„	1·9	22·3
		„ 2·3	„ 19·5

Here it will be seen that the increments vary from 5·1 to 1·9; or, if the various animals be grouped more or less according to their morphological relationships, and means taken, the numbers vary from 4·8 for the Ctenophores examined, to 2·3, or less than half the amount, for the teleost fish. With evolution of morphological structure, there would, in fact, appear to be a gradual evolution of increased power of resistance to variations of temperature. The tunicate *Salpæ* appear to form an exception, but, omitting them, we find that the temperature increment gradually diminishes as we pass first to the two medusæ examined, then to the three mollusca, then to the acraniate *Amphioxus*, and lastly to the teleost fish. As may be gathered from the last column of the table, this gradual evolution is accompanied by a gradual increase in the percentage of solids in the tissues of the organisms. Indeed, it seems highly probable that it is to this cause, rather than to the increased complexity of morphological structure, and growth of nervous control over the tissue metabolism, that the increased resistance to temperature changes is chiefly due. Thus all the animals in the above

table may be roughly divided into two classes. The first seven of them are transparent pelagic animals, in which the temperature increment is, with one exception, 3·2 and upwards, and the percentage of solids in the tissues ·6 per cent. or less. The remainder, also with one exception, *viz.*, *Tethys*, are littoral animals, and not transparent, have a temperature increment varying from 1·9 to 2·7, and contain from 11·7 to 22·3 per cent. of solids. In any case, whether the increased resistance to temperature change be ascribed to the one factor or the other, it is evident that there has taken place an evolution of this physiological characteristic, and that it has, at least to some extent, proceeded hand in hand with the morphological evolution.

In the above-mentioned animals, the respiratory activity appeared to increase regularly with the temperature, and there was no evidence that the nervous system had any special power of influencing the metabolism. It has also been hitherto generally considered that this is true for all cold-blooded animals, and that, provided these are in a state of rest, their metabolism and body temperature depend only on the temperature of their surroundings. As a matter of fact, the few data available did not altogether warrant this conclusion. Thus Moleschott [1] found the carbonic acid output of frogs at 5° to be in some cases greater than that at 10° and 15°. Also Schultz,[2] in his determinations of the carbonic acid discharge of the edible frog, *Rana esculenta*, found the metabolism at 14·4° to be less than that at 6·4°.

The writer [3] has recently made similar determinations of the relations of the respiratory activity to temperature in various amphibia and other cold-blooded animals, and has found that in hardly any case does the carbonic acid output increase regularly with increase of temperature. There exist temperature intervals over which the metabolism remains either constant, or varies but slightly. The animals were slowly warmed from 2° to 30°, or slowly cooled from 30° to 2°, and the carbonic acid discharge determined over

[1] *Untersuchungen zur Naturlehre*, 1857, Bd. ii., 315.

[2] *Pflügers Archiv*, xiv., 78.

[3] *J. Physiol.*, xvii., p. 277, 1894, and xxi., p. 443, 1897.

temperature intervals of 2·5° or 4°. By making several determinations with each animal, the average relation of respiratory activity to temperature could thus be fixed with a fair amount of accuracy. In the following table are given the temperature intervals, both on warming and on cooling, through which the carbonic acid output of the animals remained practically constant. In those cases in which the figures are placed in brackets the metabolism increased or decreased with increase or decrease of temperature, but to a much smaller extent than at temperatures above or below the limits mentioned.

Animal.	Temperature intervals of approximately constant CO_2 output.		CO_2 output at 24° / CO_2 output at 10°
	On warming.	On cooling.	
English frog (*R. temp.*) (old series)	6°-17·5°	(17·5°-12·5°)	2·3
„ „ (new series)	12·5°-15·0°	(17·5°-12·5°)	
Edible frog (*R. esculenta*)	15·0°-20·0°	(17·5°-2°)	2·1
Common toad	10°-22·5°	(17·5°-2°)	2·0
Axolotl	(2°-20°)	(20°-2°)	2·2
Newt	10°-22·5°	25°-17·5°, 15°-12·5°	1·4
Blindworm	(10°-20°)	(20°-2°)	3·1
Snail	20°-27·5°	(17·5°-15°)	2·4
Earthworm	10°-22·5°	(20°-10°)	1·9
Cockroach	—	—	4·3

In the case of the English frog, one series of determinations showed the metabolism to remain practically constant from 6° to 17·5° on gradually warming the animal, and to diminish only slightly between 17·5° and 12·5° on gradually cooling it. In another series, made three years later, the carbonic acid output remained constant for a considerably shorter interval on warming, but the rate of increase, with increase of temperature, was very much less at temperatures between 2° and 25°, than at temperatures above 25°. Of all the nine animals examined, the newt, the toad, and, strangely enough, the common earthworm, exhibited this constancy of carbonic acid output to the most marked extent. Thus in all of these animals, it remained practically

constant from 10° up to 22·5° on warming, and diminished only very slightly over similar temperature intervals on cooling. In the cockroach alone of the animals examined, did the carbonic acid output increase and decrease uniformly with increase and decrease of temperature.

To what is this want of dependence of metabolism on temperature due? It must obviously be ascribed either to a direct control of the central nervous system over the respiratory activity of the tissues, or to the tissues being themselves so constituted that their metabolism is affected by temperature in this curious and irregular manner. This second supposition appears on the face of it highly improbable, and that it is actually incorrect was experimentally proved. Thus it was found that if a transverse section were made in the medulla of the frog or toad, at the level of its lower border, the carbonic acid output now varied uniformly with the temperature and showed no intervals of constancy. If, however, the transverse section were made at the level of the upper border of the medulla, the relation of carbonic acid output to temperature was as a rule the same as in intact frogs, though occasionally irregular results were obtained. It appears therefore that there is a definite centre situated somewhere in the region of the medulla, and that this centre is able to send out impulses to the various tissues of the body, and so control their metabolism. Probably the muscles are the tissues chiefly concerned in the maintenance of this metabolism, and it might be thought that it is maintained through the agency of the ordinary motor nerves which are concerned in muscular contractions. This does not seem to be the case, however, as it was found that if doses of curare were given to frogs sufficient to paralyse all voluntary and reflex muscular movements, the carbonic acid output still varied irregularly with the temperature. It was only when excessive doses of the drug were administered that the one varied regularly with the other.

We see, therefore, that in almost all the cold-blooded animals examined, there exist intervals of temperature over which, through the agency of a nervous regulating mechanism, the metabolism remains more or less constant. When

so lowly an animal as the earthworm possesses the power of perfectly controlling its carbonic acid discharge, it is obvious that in many other members of the animal kingdom, thus far unexamined, the same condition of things may be present. It is noticeable that the intervals of constant metabolism more or less cover such variations of temperature as the animals would be ordinarily exposed to under normal conditions, except in the extreme cold of winter and extreme heat of summer. The teleological explanation of such an arrangement is obvious. If the metabolism depended absolutely on the temperature, the animal would lose much of its control over its own powers. It would of necessity be always lively in hot weather, and torpid in cold. All its movements would be regulated by the temperature of its surroundings, and not by its own wants and desires. In cold weather it could not move fast enough to escape its enemies, should they be warm-blooded ones, and in hot weather it would have difficulty in obtaining sufficient food to replace the increased tissue loss.

In the last column of the above table are given the temperature increments, or relation of the carbonic acid output at 24° to that at 10°. It will be seen that with one or two exceptions, which are probably in part accidental, these values remain at about 2·0, or about the same as those obtained in the former experiments on teleost fish. In the case of the cockroach, however, the very high value of 4·3 was obtained, or as great an effect as was noticed in the transparent pelagic cœlenterates and tunicates.

We have thus seen that in certain amphibia and other animals the nervous system has been sufficiently evolved to exert a controlling power over the tissue metabolism. The only other conditions necessary to convert these animals into warm-blooded, or homoiothermic, animals of even temperature, would therefore appear to be an increased heat production of the tissues, coupled with a modification of the external covering of the body so as to diminish the heat loss. Thus the temperature of fish and amphibia is, as a rule, only a fraction of a degree above that of their surroundings. Now the actual carbonic-acid discharge of a

bird, which has a body temperature of over 40°, is only some ten to twenty times greater than that of a frog, of equal weight, at a temperature of 30°, though it is some fifty to a hundred times as great as that of a frog at 15°. With a more efficient protection against heat loss, it therefore follows that the temperature of cold-blooded animals would be several degrees above that of their surroundings. Thus it would need a proportionately much smaller degree of heat-production to keep an animal 5° above the environmental temperature than to keep it 25°. In a few cases it has been shown that in cold-blooded animals the body temperature may be raised considerably above the external temperature. Thus Davy[1] found the temperature of the shark to be 1·3°, and of the bonito (*Thynnus pelamys*) to be no less than 10° above that of the water in which they were kept. Again, the temperature of reptiles may be raised still more considerably. John Hunter[2] found the temperature of a viper to be raised 5·6°, and Sclater[3] found the temperature of a male python on two occasions to be respectively 9·0° and 8·8°, and that of an incubating female no less than 12·7° and 20·0° above that of the air. Forbes made similar observations,[4] and found the temperature of a male python to be 6·4°, and of a female 9·3°, above the air temperature. Still again, Dutroche[5] finds that the temperature of the green lizard (*Lacerta viridis*) may be from 4° to 7° above that of the atmosphere. The high temperature which beehives may attain is well known. Thus Newport[6] found that on one occasion, when the temperature of the air was −7·5°, that of a hive was −1·1°. When the bees were disturbed by tapping the hive, the temperature was raised to 21·1° in fifteen minutes. On another occasion, when the external temperature was 1·4°, that of the hive full of active bees was 38·9°. It is obvious, however, that the bees can

[1] *Researches*, vol. i., p. 189, 1839.
[2] *Works*, Palmer's edition, vol. iv., p. 131, 1837.
[3] *Proc. Zool. Soc.*, 1862, p. 365.
[4] *Proc. Zool. Soc.*, 1881, p. 960.
[5] *Ann. des Sciences Nat.*, xiii., p. 20.
[6] *Phil. Trans.*, 1837, pt. ii., p. 253.

only produce such a considerable elevation of temperature when large numbers of them are confined in a small space. A single bee exposed to a low temperature soon becomes torpid and almost motionless.

We now come to the very interesting observations which have been recently made upon the temperatures of monotremes and marsupials. Upon the duck-billed platypus (*Ornithorhyncus paradoxus*), which is the lowest of the monotremes, and in fact the lowest member of the whole class of mammals, two observations, both on the same individual, have been made by Miklouho-Maclay.[1] As a mean, the temperature was found to be 24·8°, or only 2·6° higher than that of the water in which the animal was kept. It would therefore seem to be more closely related to the cold- than to the warm-blooded animals. The same observer also made several observations[2] upon another monotreme, *Echidna hystrix*. The mean temperature was 28·0°, that of the air being 20·0°. Semon[3] also made several determinations, and found the average rectal temperature to be 32·5°. Sutherland[4] has recently investigated the subject more fully, and has determined how the temperature of the animal is affected by that of its surroundings. The following are the means of the results obtained by him :—

Number of Observations, each on 3 to 6 individuals.	Average Temperature of Air.	Average Temperature of *Echidna*.
3	14·1°	25·7°
4	17·4°	28·2°
9	22·1°	30·2°
1	31·2°	32·2°
1	45·0°	36·6°

Here we see that the temperature of these animals is affected very considerably by that of the environment, and that by increasing the temperature from 14·1° to 45·0°, the mean temperature of the animals rose from 25·7° to 36·6°. Even these figures do not express the extreme temperature

[1] *Proc. Linn. Soc. N. S. Wales*, ix., p. 1204. [2] *Ibid.*, viii., p. 425.
[3] *Arch. f. d. ges. Physiol.*, Bd. 58, S. 229, 1894. [4] *Ibid.*

variations of which the animal is capable, as the temperature of one individual on a cold morning was found to be only 22°. Altogether twenty-seven series of observations were made on fourteen different specimens, the mean temperature being 29·4°. This animal in question thus appears to form a true intermediate stage between the cold-blooded animals on the one hand, and the warm-blooded on the other. It is able to keep the temperature of its body considerably above that of the environment, provided this be low, or considerably below it provided this be high; but it is, nevertheless, affected very largely by these external temperature changes, though not to so great an extent as the body temperature of a true cold-blooded animal.

Upon the marsupials, Sutherland made 126 observations, upon sixteen different species. The mean temperature of all of them was 36·0°, or considerably higher than that of the monotremes, but still appreciably lower than that of the true placental mammals. In the following table are given the mean temperatures of the individuals of each genus examined :—

Number of Observations.	Animal.	Temperature.
2	Wombat (*Phascolomys*) - - - -	34·1°
5	Flying squirrel (*Petaurus*) - - -	35·7°
83	Koala (*Phascolarctos*) - - - -	36·4°
—	„ (excluding females at breeding time)	36·0°
—	*Dasyures* - - - - - -	36·0°
22	Ring-tailed opossum (*Phalangista*) - -	36·6°

Even among these marsupials we see that the mean temperature varies from 34·1° to 36·6°. The extreme limits of temperature amongst individuals of the same species are even greater. Thus in healthy specimens of the koala, temperatures varying from 34·9° to 38·4°, or by 3·5°, were noticed. Now in healthy placental mammals under normal conditions the limits of temperature rarely exceed one or two degrees. The koala was also influenced by the temperature of its environment to a greater extent than is the case with placentals. Thus as a mean of three observations

at 9·7° the body temperature was 35·3°, and of six observations at 19·4°, 36·2°.

Finally, on members of the kangaroo family, four observations were made, with the following results:—

Pterogale xanthopus,	35·9°
Macropus giganteus,	36·6°
Dendrogalus grayi,	37·0°
Halmaturus bennettii,	37·1°

In the last two of these animals we see that the temperature of placental mammals was reached. For the sake of comparison, a table is appended showing the mean rectal temperature of some of the better known of these mammals.[1]

Man	37·0°	Dog	38·6°
Horse	37·8°	Cat	38·7°
Monkey	38·4°	Rabbit	38·9°
Cow	38·6°	Sheep	40·2°

These we see to vary from 37·0° for man to 40·2° for the sheep.

We have now arrived at the true placental mammals, and it remains for us to inquire how far these resemble and differ from cold-blooded animals in the reaction of their temperature and respiratory activity to changes in the temperature of their surroundings. It is generally stated that warm-blooded animals differ from the cold-blooded in exhibiting an increased respiratory activity at low temperatures and a diminished one at high temperatures, and also that the nervous system is able to regulate the heat production and heat loss so efficiently that the body temperature is practically uninfluenced by that of its surroundings. We shall see that under certain conditions both of these relations are departed from, and that thereby a connection with the cold-blooded animals is established.

As is but natural, these questions of respiratory activity

[1] These data are taken from the table given on p. 790 of Schäfer's *Textbook of Physiology*, vol. i.

and body temperature have been studied most fully in the case of man, and hence we shall refer to the results so obtained at some little length. Thus Liebermeister[1] determined the effect on the respiration of baths of various temperatures, and found that the carbonic acid output increased regularly with decrease of temperature of the water. Voit[2] exposed his subjects of experiment in a respiration apparatus to air temperatures varying from 4·4° to 30°. In order to diminish the experimental errors as much as possible, no food was taken by the subjects for sixteen hours previously, and every experiment extended over six hours. The carbonic acid output was found to diminish regularly from 210·7 gm. per six hours at 4·4°, to 155·1 gm. at 14·3°. From this point it slowly but rather irregularly rose again, till at 30° the output was 170·6 gm. Thus the respiratory activity did not vary regularly with the temperature, but reached a minimum value at an intermediate temperature. A similar reaction to temperature has been observed in other mammals. Thus Page[3] determined the carbonic acid discharge of a dog at temperatures varying from 15° to 35°, and found that the metabolism reached its minimum at 25°. Rubner[4], working with guineapigs, obtained the following results :—

External Temperature.	Temperature of Animal.	Carbonic Acid output per kils. per hour.
0°	37·0°	2·91 gm.
11·0°	37·2°	2·15
20·8°	37·4°	1·77
25·7°	37·0°	1·54
30·3°	37·7°	1·32
34·9°	38·2°	1·27
40·0°	39·5°	1·45

Here we see that the external temperature varied from 0° to 40°, and that the metabolism reached its minimum at

[1] *Deutsch. Arch. f. kl. Med.*, Bd. x., S. 75, 1872.
[2] *Ztschr. f. Biol.*, Bd. xiv., S. 79, 1878.
[3] *J. Physiol.*, ii., p. 228.
[4] *Biologische Gesetze.* Marburg, 1887.

34·9°. This table is introduced chiefly, however, in order to illustrate another point, *viz.*, the effect of the environmental temperature on that of the animal under experiment. We see that with a single exception the body temperature rose regularly, with the rise of external temperature, from 37·0° to 39·5°. This rise is almost as considerable as was observed by Sutherland in the Koala, though not nearly so great as in *Echidna*, and it goes to prove that in the guineapig, at least, the power of regulation of the body temperature is by no means perfect.

To return to the experiments on man, Loewy[1] made numerous observations in which the subject of experiment lay at rest on a sofa, and had his respiratory exchange determined when he was in a clothed and unclothed condition. In other experiments warm and cold baths were used. The chief point of these experiments lay in the fact that especial attention was paid to the presence or absence of shivering and other movements of the subject when exposed to cold. It was found that the oxygen intake remained uninfluenced by the exposure to cold in twenty cases, and was diminished in nine of them. It was increased in twenty-six cases, but in thirteen of these shivering and muscular movements were observed, and it was probable that in the other cases there were also some such movements, though not sufficient to be observed. It would therefore seem that on exposure to low temperatures increased movement is the chief if not the only means the nervous system possesses of increasing the heat production of the body. This conclusion has been confirmed by the numerous experiments recently made by Johansson,[2] as also by the fact that in the various experiments which have been made on dogs, guineapigs, rabbits and mice it was always found that the animals were more active and showed increased movements on exposure to lower temperatures. In Johansson's observations, the subject of experiment was seated or lay in bed for a certain time, and the carbonic acid output was determined. All the clothing was then removed, and the carbonic acid out-

[1] *Pflüger's Archiv.*, Bd. xlvi., S. 189, 1890.

[2] *Skandinavische Archiv. f. Physiol.*, Bd. vii., S. 123, 1896.

put again determined over several intervals of a quarter or half an hour. The subject then resumed his clothing, or lay covered up in bed again, the carbonic acid being further determined. Johansson found, as had Loewy, that the respiratory activity was considerably increased if shivering and other movements were induced by the cold, but concluded that in those cases in which these movements were kept in abeyance by an effort of will, the carbonic acid output was practically uninfluenced by the temperature of the surroundings. In the following table are shown the results he obtained in support of this contention. The temperatures given are those of the air in the respiration chamber.

Temperature of Air.	Per cent. change of CO_2 output during cold period.	Fall of rectal temperature during cold period.
13·7°	− 8·9 per cent.	—
14·6°	+ 12·2 ,,	− ·8°
15·3°	+ 13·3 ,,	− ·9°
16·7°	− 5·1 ,,	—
18·8°	+ 1·3 ,,	− 1·1°
19·8°	+ ·8 ,,	− ·6°
20·6°	+ 1·6 ,,	− ·68°
20·7°	+ 13·2 ,,	− ·2°
21·5°	+ 5·5 ,,	− ·5°

From these values it will be seen that on an average only 3·8 per cent. more carbonic acid was discharged when the subject of experiment was unclothed, than when he was clothed. Also it would appear that the respiratory activity was no greater when the temperature of the air in the respiration chamber was low, than when it was 7·8° higher. It is noticeable, however, that the air temperature had an appreciable influence on the actual body temperature of the subject. Thus with lower air temperatures, the fall of body temperature was from ·8° to 1·1°, and with higher, only ·2° to ·68°.

As a whole, therefore, these experiments on man and the lower animals prove that the power of regulating the body temperature is not by any means perfect, and there is no very conclusive evidence of the existence of a special

mechanism which can bring about an increased metabolism and heat production of the muscles and other tissues of the body, apart from actual muscular movements. Thus, if these are controlled by an effort of the will, as in these experiments on man, the body temperature seems to fall appreciably; whilst in the guineapig, where, of course, no such voluntary control is exercised, we have seen that the nervous system is unable to keep the body temperature constant. In the long run, however, it appears that the nervous heat regulating mechanism can perform its function more efficiently, and that it is only when the animal is exposed to somewhat rapid variations of external temperature that it is much at fault. Thus, as the result of numerous observations by Davy,[1] Rattray,[2] Crombie[3] and others, it has been found that the body temperature of man in the tropics is only a fraction of a degree above that observed in temperate zones. In arctic regions also, it suffers no diminution. Under these circumstances, the body must obviously be losing much less or much more heat than in temperate zones, and yet the nervous system is able to counterbalance the varying loss.

We thus see that the warm-blooded animal is constituted similarly to the cold-blooded one in some respects. Increase of temperature does tend to send up the body temperature, and to bring about an increased metabolism, but it is only under somewhat exceptional circumstances that the counterbalancing nervous influence is unable to more or less effectually cope with this ever present tendency. Under abnormal conditions the mechanism may be totally at fault. Thus numerous instances are recorded of drunkards who have been left exposed on cold nights, and in whom the body temperature has fallen to as low as 24·0°, 24·7° and 25·0°, but with subsequent gradual recovery to the normal temperature.[4] In cases of fever again, the heat regulating mechanism is also found wanting; in some cases, as of

[1] *Phil. Trans.*, 1850, p. 437.
[2] *Proc. Roy. Soc.*, vol. xviii., p. 526, 1870.
[3] *Indian Ann. Med. Soc.*, vol. xvi., p. 550, 1873.
[4] *Vide* Schäfer's *Text-Book of Physiology*, vol. i., p. 821.

acute rheumatism and scarlet fever, to such an extent that death would appear to ensue directly from the great rise of temperature causing coagulation of some of the proteids of the tissues. Still again, in the case of certain hibernating mammals, we know that the nervous system, though still in perfect working order, permits the temperature of the body to fall as low as 2°, and to be but a fraction of a degree above that of the surroundings. Yet in these cases the heat regulating mechanism is only temporarily in abeyance. If irritated, and caused to awake from their torpor, these animals are able in a very short space of time to raise their body temperatures through a considerable temperature interval. Thus Pembrey and Hale White[1] record a case in which a dormouse was observed to raise its temperature from 13·5° to 35·75° in an hour's time. The existence of these hibernating animals proves that there is no absolute barrier between the warm- and cold-blooded animals. Non-hibernating warm-blooded animals may, indeed, for the time being, be practically converted into cold-blooded ones by the administration of curare, or by section of the spinal cord in the cervical region. In animals so treated, the carbonic acid output and body temperature rapidly fall, and only rise again on exposure to artificial warmth.

Just as there has been a physiological evolution in the direction of increased body temperature as we pass from the lower orders of mammals to the higher, so we find a somewhat similar condition of affairs amongst the various orders of birds. Thus of the Ratitæ, the lowest of the orders, the cloacal temperature of the ostrich was found by Hobday,[2] as a mean of observations on five different specimens, to be only 37·3°, whilst the temperature of the emu was found by Le Souef[3] to be 39·5°. Of the orders of Anseres, Columbæ and Galli the following data have been obtained.[4]

[1] *J. Physiol.*, vol. xix., p. 477.
[2] *Journ. Comp. Path. and Therap.*, vol. ix., p. 286, 1896.
[3] *Vide* Sutherland's paper, *ibid.*
[4] *Vide* Schäfer's *Text-book of Physiology*, vol. i., p. 791.

Bird.	Average Cloacal Temperature.	Bird.	Average Cloacal Temperature.
Pigeon - -	40·9°	Duck - -	42·1°
Goose - -	41·7°	,, - -	43·6°
Fowl (common) -	41·6°	Pheasant - -	42·6°
,, - -	42·8°	Turkey - -	42·8°
		Guinea-fowl -	43·3°

With these birds the average cloacal temperature is only about 42°. In the Passeres, however, which form the highest order of the class Aves, the temperature is, as a rule, a degree or two higher.

Bird.	Average Cloacal Temperature.	Bird.	Average Cloacal Temperature.
Sparrow - -	42·1°	Fieldfare - -	43·7°
Thrush - -	42·8°	Swift - -	44·0°
Yellowhammer -	43·2°	Great titmouse -	44·0°
Redwing - -	43·3°		

Why there should be this gradual rise of temperature accompanying the increasing complexity of morphological structure is by no means clear. That the two conditions do not necessarily go hand in hand is shown by the above quoted data on the temperatures of certain of the higher mammals. From these it may be seen that the temperature of man is lower than that of any of the other mammals mentioned, and no less than 3·2° lower than that of the sheep. A high body temperature is certainly an advantage in one way. Thus the more it is raised above that of the environment, the easier is it for the nervous system to keep it approximately constant. On the other hand it obviously entails a higher metabolism and increased consumption of food, a weighty drawback in view of the ever present struggle for existence. Probably the temperature is to some extent bound up with the size of the animal. Thus the smaller this is the more would variations in the external temperature tend to produce variations in body temperature, and the more sensitive would the heat regulating mechanism need to be in order to keep the body temperature constant. Thus if the above data on birds be re-examined, it will be

seen that the members of the lowest order, *viz.*, the ostrich and emu, are also the largest individuals upon which observations were made, whilst those of the highest order, the Passeres, are the smallest. This apparent interdependence of size and body temperature may be a coincidence, but it is nevertheless sufficiently striking to be worthy of mention.

Just as in morphology we know that, according to the law of von Baer, the developmental history of the individual to a certain extent recapitulates the developmental history of the race, so also does a somewhat similar condition of affairs exhibit itself in regard to the physiological question under discussion. The embryonic warm-blooded animal, which has descended from a cold-blooded ancestor, is itself cold-blooded. Thus the physiological evolution of the chick, in relation to its reaction to external temperature, has been recently worked out very carefully by Pembrey, Gordon and Warren.[1] On determining the respiratory activity of eggs at various stages of incubation, these observers found that the developing chick, during the greater part of its period of incubation, responded to changes of external temperature like a cold-blooded animal. About the twentieth to twenty-first day, however, an intermediate stage was noticed, in which there was no marked response in either direction. When the chick was hatched, this neutral stage was succeeded by a stage in which it reacted as a warm-blooded animal. On lowering the external temperature, it now showed more active muscular movements, and gave out an increased amount of carbonic acid. Guinea-pigs, shortly after birth, were found by Pembrey[2] to react in a similar way, but mice, rats and pigeons, which are born blind, naked and helpless, react in the same way as cold-blooded animals. On exposure to cold their carbonic acid output is considerably decreased, and their body temperature also falls. The power of heat regulation was found, however, to be well developed in mice by the tenth day after birth, and in pigeons by the fifteenth to sixteenth day. These animals had then attained their full and true status as warm-blooded animals.

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[1] *J. Physiol.*, xvii., p. 331. [2] *Ibid.*, xviii., p. 363.